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At a recent meeting the Council of the Association voted to raise annual regular dues to \$25 for full professors and nonacademic members as a step toward reducing the deficit for the current fiscal year. The Council agreed that the increase should be put into effect starting with renewal notices sent out after the end of January 1973.

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Martyrs, Myths, and the Massacre: The Background of St. Bartholomew

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HOW, FROM A DISTANCE of four hundred years, can we obtain a clear view of the Massacre of St. Bartholomew? The refractory powers of time always present difficulties, but in this case our vision is further distorted by a screen of false and conflicting evidence and by an endless stream of partisan debate. Perhaps the answer is that we should discard altogether the idea of describing some objective set of circumstances independent of ideological presuppositions and the passions aroused in witnesses and interpreters. Perhaps we should try rather to restore the event to its various contexts, conceptual as well as historical, and from a point of view that accommodates political and religious consciousness as well as social reality, that recognizes the mythical as well as the historical dimension. For it is upon some such symbolic level that the historical significance of events is to be found.

Over the past four centuries the Massacre of St. Bartholomew has presented many faces. It has been seen as a sensational explosion of violence fired by a half-century of mounting religious hatred; as a tragedy so shocking, according to one contemporary witness, that posterity would never believe it; as a turning point in a great world conflict; as a storm center of religious polemic and a seedbed of political theories; as a legend of gigantic proportions promoted by publicists and sanctioned by men of letters; as a puzzle and topic of debate for generations of historians; as an occasion for Catholic masses, Protestant lamentations, and historical conferences.¹ It was all these and more. It was also an archetypal occurrence that transcended its

This paper is an offshoot of two projects, a biography, *François Hotman, A Revolutionary's Ordeal*, forthcoming from Princeton University Press, and a study in progress of sixteenth-century propaganda. A shorter version of this paper was read at a meeting of the Newberry Library Renaissance Conference in May 1972 devoted to "The Massacre of St. Bartholomew in Historical Perspective."

¹ The most important source material is the anonymous pamphlet literature, which is fairly exhaustively listed in the following publications: Robert Lindsay and John Neu, *French Political Pamphlets, 1547-1648* (Madison, 1969); the *Catalogue de l'histoire de France*, 1 (Paris, 1845), which is a listing by shelf mark of the Bibliothèque Nationale (BN), principally the range Lb.³³ to Lb.³⁵; the catalog of the Bibliothèque de l'Arsenal, unfortunately unpublished, in nine MS volumes, 8°H. 12868; and F.-A. Isambert, ed., *Recueil général des anciennes lois françaises*, 12 (Paris, 1822-33), numbered consecutively.

historical context—transcended it not only in the direction of the future (by endless debates over premeditation, guilt, and consequences) but also in the direction of the past (by seeming to symbolize, summarize, and confirm long-standing fears and anticipations and indeed to repeat earlier misfortunes on a grander scale). It was, in other words, an almost generic human experience that came as no surprise in the event, that followed a familiar pattern in its course, and that would be relived in various ways afterwards.

In order to suggest the basis and dimensions of this historical epiphenomenon, we must look beyond the conventional narrative and diplomatic sources that help to show (in Ranke's view) "what really happened." One of the most convenient as well as fashionable procedures is to choose some analyzable model. This seems to be all the more appropriate since the model in this instance does not have to be imported from any of the more structured social disciplines. On the contrary, it is suggested, indeed imposed upon us, by the historical context of the sixteenth-century wars of religion. Here is a clear case of history repeating itself, or at least of men seeing it as a repetition. For the witnesses, participants, and interpreters of the events of late summer 1572 knew what the phenomenon was practically before it happened: it was not a "tumult" or a "disorder" or the suppression of a "conspiracy" as various observers supposed; it was a massacre, by no means unexpected and not even the first in that generation. And they knew what part they might ultimately have to play: it was that highly stylized and stereotyped role called "martyrdom," the most exalted and yet in some ways the simplest form of sainthood. The fundamental psychological model for this phenomenon, then (again with a nominal bow to intellectual fashion), was what can only be called the martyr complex.²

The remaining problem is where to find reflections of this model, or in other words, how to gain access to this aspect of the Protestant conscience. The most obvious and direct source would seem to be that most human and down-to-earth variety of Protestant historiography, the martyrology. The tradition of latter-day martyrs was inherent in Protestant self-consciousness from the beginning—Luther's stand at Worms in 1521, so reminiscent of Hus's a century before, was potentially that of a martyr—but not for another generation did this tradition take a consciously literary form. The first fruits came within a few years and showed a pronounced family resemblance. Most significant were Jean Crespin's *The History of the Martyrs* and the first Latin version of John Foxe's *Acts and Monuments*, both appearing in 1554, in Geneva and Strasbourg respectively. Ludwig Rabe's *Stories of God's*

² A convenient bibliography on the question of martyrdom may be found in W. H. C. Frend, *Martyrdom and Persecution in the Early Church* (New York, 1967). In all the massive literature I find no useful studies from a psychohistorical or sociohistorical point of view. General Catholic treatments—of which the best is perhaps Henri Leclercq's in Fernand Cabrol and Henri Leclercq, eds., *Dictionnaire d'Archéologie chrétienne et de liturgie* (Paris, 1924–53), vol. 10, pt. 2—tend to be so rigidly legalistic and orthodox that Protestant experiences do not even come into the discussion.

Chosen Witnesses and Martyrs, published in Strasbourg in 1552, antedated these two but was limited to martyrs in antiquity until later, derivative editions. Also related were Heinrich Pantaleone's *History of the Martyrs*, Johann Sleidan's great history of the Reformation, published in Strasbourg in 1555 and soon translated into German, French, and English, and Matthias Flaccius Illyricus's *Catalog of the Witnesses of Truth*, published in Basel in 1556.³

Though independently conceived, these works were all products of the international Protestant community and had a collective, to some extent cooperative, character. The links between Geneva and Strasbourg, the two leading Calvinist centers, were particularly strong and were strengthened by the presence of the exiles from Marian England. Crespin was in close touch with Calvin's friend Sleidan, drew upon Sleidan's book, and in 1556 published a French translation of it in Geneva. For later versions of his own *History* Crespin drew also upon Foxe's concomitantly growing *Book of Martyrs*. Foxe himself had gone to Strasbourg in 1555 and then settled in Basel to continue his work, but he continued to receive materials from his friend Edmund Grindal, and no doubt indirectly from Sleidan. Foxe also made use of Flaccius Illyricus's *Catalog*, which was printed in 1556 by Foxe's own publisher, Johann Oporinus. In addition Foxe's work was continued by Pantaleone, who was also Sleidan's German translator. The relation of Rabe, a leading theologian in Strasbourg's Lutheran congregation, is not known, but in any case it seems clear that the first generation of martyrologists constituted something approaching a literary circle, a kind of Protestant pleiade of the exile circuit, which drew upon a common fund of experience, a common ideological commitment, a common historical perspective, and a common reliance upon what Foxe called the "miracle" of printing.⁴

In many respects the groundbreaking work was Crespin's, and for France, certainly, it provided the model. Crespin was himself an exile from the Netherlands and, along with his friend François Baudouin, had barely escaped from his native Artois with his skin. That was in 1545. Afterwards Crespin settled in Geneva, set up his printing press, and launched into his life's work. The *History of Martyrs* was based on a wide range of printed and unprinted sources and on several other independent works, including those of Foxe, Antoine de la Roche Chandieu, and François Hotman, the first historian of the Massacre of St. Bartholomew. Crespin also

³ Ludwig Rabe, *Der heiligen aus erwählten Gottes Zeugen Bekennen und Martyren* (Strasbourg, 1552), with a treatment of the modern period in the expanded edition (Strasbourg, 1571); Jean Crespin, *Le Livre des martyrs* ([Geneva], 1554); John Foxe, *Commentarii rerum in ecclesia gestarum* (Strasbourg, 1554), dedicated to Christoph von Württemberg, Aug. 31, 1554; Heinrich Pantaleone, *Martyrum historia* (Basel, 1563); Johann Sleidan, *De Statu religionis et reipublicae, Carolo Quinto, Caesare, Commentarii* (Strasbourg, Sept. 1556); Matthias Flaccius Illyricus, *Catalogus testium veritatis* (Basel, 1556).

⁴ For some of these complex interrelations, see G. Moreau, "Contribution à l'histoire du livre des martyrs," *Bulletin de la Société de l'histoire du protestantisme français*, 103 (1957): 173-99.

drew on the *Ecclesiastical History of the Reformed Churches of France*, long attributed to Theodore Beza. The *History of Martyrs* appeared in successively augmented and altered editions between 1554 and 1570 (the last published by Crespin) and thereafter was continued by Simon Goulart, the prolific polemicist who issued the most comprehensive edition in 1619, when he had succeeded Beza as head of the Genevan church. This book is the centerpiece, as far as France is concerned, of the modern martyrological canon.⁵

Perhaps the most fundamental theme of this whole genre was, in Crespin's words, "the conformity of the modern history of the martyrs with that of antiquity." This parallel was perhaps what Lefèvre d'Étaples had in mind when he began to compile his *Agonies of the Martyrs*, as did Rabe, who referred explicitly to the "proto-martyrs" of antiquity. "The memory of the first persecutions," as Chandieu put it, "is a school that teaches how to remain true to one's calling." Indeed the purpose of collecting these biographical accounts included all of the basic ingredients for the humanist prescription for history. They offered "consolation," as Crespin wrote in his first preface; they constituted a treasury of *exempla* for imitation and a kind of moral and anagogical "mirror" for Christians; and they were commemorative, preserving for posterity the "deeds and writings" of exemplary men of faith.⁶

Yet there is no doubt that these humanist commonplaces were transformed by their conscription into the service of militant Protestantism. The biographical form of martyrology resembled less the *de viris illustribus* of classical tradition than the *vitae sanctorum* of the medieval Church. In each biography Crespin wrote, it was his intention to describe the doctrine as well as the life of the martyr and above all his "happy ending"; and to

⁵ Besides the invaluable edition by Daniel Benoit of Crespin and Simon Goulart, *Histoire des martyrs* (Toulouse, 1885–89), see Arthur Piaget and Gabriel Berthoud, *Notes sur le livre des martyrs de Jean Crespin* (Neuchâtel, 1930); C.-L. Frossard, *Le Livre des martyrs de Jean Crespin* (Paris, 1880); and more generally, Ferdinand Vander Haeghen et al., *Bibliographie des martyrologies protestants néerlandais*, 2 (La Haye, 1890); also the article on Crespin in Eugène Haag and Emile Haag, eds., *La France Protestante* (2d ed.; Paris, 1877–88); the notes to Calvin's correspondence in Calvin, *Opera quae supersunt omnia*, ed. J. W. Baum, Eduard Cunitz, et al. (Strasbourg, 1863–1900), vols. 38–39; and J.-F. Gilmont, "Une édition inconnue du martyrologe de Jean Crespin," *Bibliothèque d'Humanisme et Renaissance*, 30 (1969): 363–71. The latter article and that by Moreau, "Contribution à l'histoire du livre des martyrs" (p. 174), refer to the work of E. E. Halkin and his students, which is important though devoted mostly to the martyrs of Belgium and the Netherlands. On Rabe's book, see Robert Foncke, *Duitse Vlugschriften van de Tijd over het Proces en de Terechstelling van de Protestanten Frans en Nikolaas Thys te Mechelen* (Antwerp, 1937), 60–65; and William Haller, *Foxe's Book of Martyrs and the Elect Nation* (London, 1963), 55–73. The other related works are Antoine de la Roche Chandieu, *Histoire des persecutions et martyrs de l'église de Paris depuis l'an 1557* (Lyons, 1563), BN, Rés. Ln.²⁵. 91; *Histoire ecclésiastique des églises réformées au Royaume de France*, ed. J. W. Baum and Eduard Cunitz (Paris, 1883); [François Hotman], *De Furoribus Gallicis* ("Edinburgh" [probably Basel], 1573); and [Hotman], *Gasparis Colinii Castelonii, magni quondam Franciae amiralii vita* (hereafter *Vita Colinii*) (n.p., 1575).

⁶ Crespin, *Histoire des martyrs*, vol. 1, prefaces of 1570 and 1554, *passim*; [Lefèvre d'Étaples], *Agones martyrium mensi ianuarii. Libro primo* (Paris, 1529) (no more published); Rabe, *Heiligen aus erwählten Gottes Zeugen Bekennen und Martyren* (1571), vol. 2; Chandieu, *Histoire des persecutions*, xiii.

suit this purpose Crespin did not hesitate to stretch the already flexible standards of sixteenth-century editorship, improving in various ways upon the texts of even original documents. Chandieu took pride in declaring his "fidelity" to truth and especially to his sources, but his partisanship was still more flagrant. The purpose of his book was both to afford "profit" to his brothers and to demonstrate the justice of his "Cause" to the "poor ignorant ones" outside of it. The book was quite literally a call to arms. "We are not in this world to rest," he declared, "but rather to fight."⁷

The most direct source of inspiration for these martyrologies was clearly the Protestant view of ecclesiastical history, as variously expressed by Luther, Melanchthon, and Calvin. "From all authors and histories," declared Flaccius Illyricus, "it is evident that our church . . . is truly ancient and takes its origin from the time of Christ and the apostles."⁸ In the subsequent life of this true church the persecutions of the faithful constituted a major source of continuity, which according to Crespin paralleled the progress of Grace on earth and indeed represented its carnal counterpart. "The blood of martyrs is the seed of the church" was the invariably quoted motto of Tertullian, and its life-giving force was continuous over the centuries. Moreover, like Christian tradition in general and in sharp contrast to the national orientation of pagan historiography, the history of martyrs was truly universal in scope and included, as Crespin remarked, "all conditions, ages, sexes, and nations."⁹ It differed from medieval historiography, too, especially from the saints' lives with which it had undeniable resemblances, in that it was concerned not with the idolatrous relics but with the spiritual legacy of the faithful—with their "words and deeds." Like the view of ecclesiastical tradition held by Melanchthon and Flaccius Illyricus, the view of the Crespin-Goulart martyrology was not merely "human" but doctrinal or confessional.

Between the first, archetypal age of Christian martyrs and the moderns came a time that seemed darker to Crespin than it did to many of the most critical of humanists (which may serve as a reminder, though surely not a revelation, that the myth of the "Dark Ages" was as much a creation of the Reformation as it was of the Renaissance). After the primitive church came the papal monarchy—"la monarchie papistique" is Crespin's phrase—and then, at a still lower level, a third age ridden with Scholasticism, canon law, relic worship, and other forms of idolatry.¹⁰ Throughout this period there was a tradition of "pure religion," but it was tenuous and took the form only of scattered "witnesses to the truth"—"precursors" is the word modern

⁷ Chandieu, *Histoire des persecutions*, xxviii.

⁸ Flaccius Illyricus, *Catalogus testium veritatis*, preface.

⁹ Crespin, *Histoire des martyrs*, 1:1; Chandieu, *Histoire des persecutions*, lxii. See also Hans Campenhausen, *Die Idee der Martyrium in der alten Kirche* (Göttingen, 1964), and the references there to Augustine, Luther, and Calvin.

¹⁰ Crespin, *Histoire des martyrs*, 1: 41; see also Heinrich Bullinger, *Origines errorum* (Zurich, 1549).

historians prefer for this still no less mythical phenomenon. Finally came the revival of true doctrine begun by Luther, which brought with it, according to Crespin, a renaissance of martyrdom as well.

SO MUCH FOR MARTYROLOGY as a model. The original, martyrdom itself, is more difficult to define. A legal concept, a value judgment, a psychological condition, a social role, a weapon of propaganda—again it was all these, and more. The idea, if not the terminology, long antedated Christianity. As early as the fifth century, for example, Athenians killed in war were assured of deification; and this notion of dying in battle for a just cause has always been associated with that of martyrdom.¹¹ But it was Christianity that adopted the Greek term *martys* to this general behavior pattern. Although the original signification was lost in the Latin West, Protestant martyrologists quite consciously, in the spirit of Biblical humanism, restored it and identified the martyrly condition with one “testifying” to the faith.¹² It was not the fact of death, in other words, but the inner attitude that admitted one to the pantheon of martyrs and so, though theologically this could not be explicit, to the certainty of salvation as well as earthly immortality. The vital importance of the act of testifying in early Protestantism is further highlighted by the counterconcept—“Nicodemitism” was the term coined by Calvin in 1543—referring to the concealment of one’s faith, one of the most heinous of all sins.

Martyrdom was a highly conventional as well as highly painful process: it was a form of mimesis—*imitatio Christi* with a vengeance. And to follow Christ, “captain of the martyrs,” as Crespin put it, entailed a heavy weight of ritual, rhetoric, etiquette, and symbolism, as reflected in interrogations, confessions of faith, execution scenes, crowd reactions, and contemporary graphic representations. The stereotyped character of the role is clearly evident in the martyrologies—not only in the “ten marks of the martyr,” which Crespin established for purposes of identification and as the Protestant equivalent of canonization, but also in the unarticulated categories, including the status of the accused and the types of punishment, which modern scholars have extracted and applied in an almost quantitative fashion.¹³ These categories were confirmed, and in some cases created, by the more or less predictable reactions of established authority, which in France

¹¹ Ernst H. Kantorowicz, “*Pro Patria, Mori* in Medieval Political Thought,” *AHR*, 56 (1950–51): 472; included also in his *Selected Studies* (New York, 1965), 308.

¹² See H. A. M. Hoppenbrouwers, *Recherches sur la terminologie du martyre de Tertullien à Lactance* (Nijmegen, 1961); and Carlo Ginzburg, *Il Nicodemitismo* (Turin, 1970).

¹³ Crespin, *Histoire des martyrs*, preface of 1570. A modern example is Piaget and Berthoud, *Notes sur le livre des martyrs*, drawing upon Vander Haeghen, *Bibliographie des martyrologies*. Such attempts deserve to be followed up by more statistically grounded studies, not only along the lines suggested by Geoffrey Nuttall, “The English Martyrs, 1535–1680,” *Journal of Ecclesiastical History*, 22 (1971): 191–97, but also, and indeed at the same time, along those of Lacey Baldwin Smith, “English Treason Trials and Confessions in the Sixteenth Century,” *Journal of the History of Ideas*, 15 (1954): 471–98.

was determined to uproot all heresy. The result was to intensify the impression of Protestants that they were recapitulating the experiences of early Christians—confronting the same style of interrogation, the same inseparable charges of blasphemy and sedition, and the same sorts of repression and punishments. It may be added that they also anticipated the same kind of vengeance, that is, the decline and fall of the persecuting state; for such was the judgment often expressed by Protestants about the fate of France in the later sixteenth century.¹⁴

It is not going too far to suggest that the coherence of the modern tradition of martyrs was created as much by external pressures as by the sharing of a common ideology and a common "Cause."¹⁵ Catholic attempts to discredit individual martyrs only provided further publicity. Stories about last-minute recantations were always being circulated, remarked Erasmus, rejecting one such rumor about Louis Berquin, the first notable French martyr (who indeed, against Erasmus' advice, had chosen to stand upon his conscience).¹⁶ Catholics were always as ready to attribute a "Virgin, intercede for me" to a man about to be killed as Protestants were to find a martyrly defiance; what counted most, however, was the fact of execution. The Catholic solution was to create a kind of countermythology about heretics and, ultimately, an "antimartyrological" tradition to undo the work of Crespin and his fellow authors.¹⁷ In ideological wars, it seems, demonology has often been the response to hagiography.

This dialogue was also pursued on the official level. From the mid-1520s Protestants were plagued by a wide range of repressive legislation that constituted a kind of mirror image of their own propaganda and was an almost continuous effort to regulate their behavior on every level. The legislation was not always consistent (alternating between prescribing banishment and forbidding emigration, for example, and between controlling the printing press and abolishing it altogether), but its tone was unmistakable. Except for intermittent periods of crisis or compromise, it reflected an almost totalitarian, off-with-their-heads attitude toward heresy and disobedience that was hardly less intense than the fanaticism of the Huguenots themselves. The royal ordinances are filled with repetitive orders banning weapons, expelling vagabonds and such dangerous elements, forbidding Protestants the right to inheritance, officeholding, and burial, and prescribing death for the printing, selling, or even possession of heretical or seditious literature and for the convening of "illicit assemblies." Again and again government policy was declared to be "the extirpation and extermination" of

¹⁴ See, for example, Chandieu, *Histoire des persecutions*, lxxv.

¹⁵ "La CAUSE," i.e. of the Huguenots, became a central theme of debate between Protestants and Catholics immediately following St. Bartholomew; see especially Pierre Charpentier, *Epistola ad Franciscum Portum* (n.p., 1572); Franciscus Portus, *Responsio* (n.p., 1573); and Jean de Montluc, *Oratio* . . . (Paris, 1573).

¹⁶ Erasmus, *Opus epistolarum Des. Erasmi Roterdami*, ed. P. S. Allen and H. M. Allen, 8 (Oxford, 1934), no. 2188.

¹⁷ Jacques Severt, *L'Anti-Martyrologie* . . . (Lyons, 1622).

heresy.¹⁸ The most spectacular expression of official disfavor, of course, was the unending series of executions staged as public exhibits at strategic spots, especially market places like the Place de Grève and the Place Maubert in the university section of Paris.¹⁹ It was this campaign, more than anything else, that gave French Protestantism its persecution complex and Protestant propaganda its paranoid style.

From the time that the winds of Lutheran doctrine first reached France, bracing to some, to others bringing an odor of heresy and sedition, the production of martyrs became a familiar feature of religious opposition. In many cases, of course, the pattern of early Christianity was consciously and pridefully followed. From the execution of the self-styled hermit Jean Vallière on August 8, 1523, the honor roll of this special echelon of the elect grew and was carefully preserved by religious chroniclers and martyrologists. From 1534—the notorious “year of the placards,” when heretical posters were distributed in the streets of Paris and even affixed to the king’s chamber door in the castle of Amboise—the official reaction became more intense, and within a few months the list of martyrs increased considerably. It was at this time, Crespin reported, that the practice of cutting off the tongues of heretics before execution was initiated, although this symbolic act was notably unsuccessful in silencing French Protestantism in general.²⁰

It was the “miracle” of printing that gave Protestantism its voice, but this same miracle also produced one most unattractive offshoot—the institution of preventive censorship. Book burning was by no means a Catholic monopoly, as Luther’s sensational act of throwing the corpus of canon law into the fire demonstrates, but it was specifically Protestant literature that furnished most of the fuel at the beginning. Bearing witness in the embarrassingly public form of print often entailed equally drastic retaliation by established authority; and condemning and destroying books may well be regarded as one of the preliminaries of martyrdom itself, since more than one printer and propagandist followed his books into the flames. So it was with Berquin, so it would be with Étienne Dolet (that unwilling “martyr of the Renaissance” who did in fact recant),²¹ and so it would have been with Calvin and Beza if they had returned to France.

One crucial factor in the ideological polarization of society was the alienation of the younger generation. A year before the affair of the placards there was an indication of this alienation in an edict that sought to bring greater discipline into the university of Paris. Specifically the order banned “the impudent books of the heretics” and called for interrogation of younger students if such books were found in their possession. The edict

¹⁸ Isambert, *Recueil*, vol. 12: no. 128 (June 10, 1525), no. 211 (Jan. 29, 1534), no. 367 (July 23, 1543), no. 382 (July 24, 1557).

¹⁹ See John Viénot, *Promenades à travers le Paris des martyrs, 1523–59* (Paris, 1913).

²⁰ Crespin, *Histoire des martyrs*, 1: 297.

²¹ R. C. Christie, *Etienne Dolet, The Martyr of the Renaissance: 1508–1546* (London, 1899).

also frowned upon long whiskers (*prolixa barba*) worn by masters, and it was later derided by Beza, a student in these years, as the "edict of the beards."²² In the course of the reaction to the placards (in which university persons were deeply implicated), Francis I issued a special warning to the university community about the danger of heresy. "I pray that you . . . be especially solicitous of the youth," he told the faculty in the spring of 1535, "and see that they are well instructed and indoctrinated [*indoctriner*] so that they do not fall into the evil and forbidden opinions."²³

This incipient conflict of generations affected the family as well as education. One striking but not unusual example is that of François Hotman, who was converted to the "new opinions" at precisely the time that his father was taking his place on that special tribunal of the Parlement of Paris, the *Chambre ardente*, which took over the official campaign of suppression in 1547. The conflict was so intense that the next spring the younger Hotman made his decision—after a crisis very much like that of his friend Beza a few months later—to break ties with his home and go into exile; and he made quite clear the significance of the martyr complex in his choice. "My father ended his career by oppressing more than a thousand martyrs," he later told Melanchthon. "As long as I was with him he tried to keep me forcibly from impiety, but God kept me for His church, and here I intend to spend the rest of my life." He denounced the way of the Nicodemite and chose Calvin, quite literally, as his new "father." "He [Hotman] abandoned the hope of a fine inheritance," Calvin later told Heinrich Bullinger, "in order to fight for Christ."²⁴ A contemporary scholar has suggested that the martyr is a kind of religious adventurer,²⁵ and it was in some such spirit that Hotman, like many another young man in these years, committed this act of rebellion.

In the spring of 1549 Hotman left Geneva (in the company of Beza, who had just decided not to go into the printing business with Crespin) to take a position in the Calvinist academy of Lausanne. Even here Hotman was not out of reach of the royal campaign of persecution. Less than four years later five students from Lausanne, on their way to join congregations in southern France, were imprisoned by the Catholic authorities of Lyons and became a *cause* more *célèbre* than that of the placards. Despite Calvin's efforts and despite pleas from various Swiss cities these young men were condemned in 1553 and, one by one, were burned at the same stake. Crespin

²² Beza to Maclou Popon, May 7, 1542, in Beza, *Correspondance*, ed. Fernand Aubert and Henri Meylan (Paris, 1960-), 1: 43.

²³ Francis I, quoted in C. E. Bulaeus, *Historia Universitatis Parisiensis* (Paris, 1673), 6: 252-53.

²⁴ François Hotman to Melanchthon, May 24, 1556, BN, MS, Collection Dupuy, vol. 797, fol. 212v. The record of the elder Hotman's activities has been published by Nathaniel Weiss, *La Chambre ardente* (Paris, 1889); the son's conversion and flight may be followed through his correspondence with Calvin, published in the *Opera*. See Calvin to Bullinger, Nov. 25, 1549, *Opera*, vol. 44, no. 1324.

²⁵ Michael Walzer, *The Revolution of the Saints* (London, 1965), 9. This study contains many interesting suggestions concerning the psychology of martyrdom.

publicized the fate of these latter-day "martyrs of Lyons" by printing their prison correspondence with Calvin in the first edition of his *History*, which appeared the following year (and which provided Sleidan with his account).²⁶ Whether these letters were authentic or assembled in some way by Crespin himself, they provided most effective Protestant propaganda and took their place in the martyrological tradition.

THE CREATION OF MARTYRS *seriatim* was alarming enough, especially when it was the result of institutional persecution, but far worse was the creation of martyrs *en masse*. According to a famous distinction of the Huguenot historian Agrippa d'Aubigne, massacre victims, though their names often went unrecorded, constituted a second type of martyr, and these were still more disturbing to the Protestant conscience.²⁷ The first sensational episode was the notorious persecution of the Waldensians in Mérindole, which Crespin called "as memorable as anything within the memory of man" and in fact thought worthy of a separate volume.²⁸ In 1545 a veritable campaign of extermination was waged in which twenty-two villages were destroyed and hundreds of persons killed; others were put to flight, and many went to Geneva, as was becoming increasingly common. This episode, together with the work of the *Chambre ardente*, haunted Protestants with the prospect of martyrdom or exile, which represented a kind of political martyrdom. Further incidents and the repressive legislation of the 1550s served to magnify such fears among the Huguenots, as they were beginning to be called, as well as to bring civil war closer.

Such fears played a part, too, in that complex set of uprisings referred to as the Conspiracy of Amboise, which such Protestants as Hotman, Beza, and Crespin looked upon as in effect the opening phase of the wars of religion. The conspiracy, brewing already in the fall of 1559 after the death of King Henry II, aimed at breaking the power of the Guise family by gaining possession of the young King Francis II. It was further stirred up by the famous trial of Anne du Bourg, who was executed at the end of that year.²⁹ In his responses to interrogators and in his last words, widely publicized by Crespin and others, du Bourg provided a *locus classicus* not only for Huguenot political propaganda but also for the tradition of martyrs.

The conspiracy itself was a fiasco. Hotman, one of the conspirators as well as the historian of this attempt to overthrow the Guise "tyranny," described the terrible fate of the captured "rebels." Some of them were hanged from the parapets of the castle of Amboise and others decapitated, and Hotman quoted in particular the words of his old friend the Sieur de Villemongis:

²⁶ Crespin, *Histoire des martyrs*, 2: 595.

²⁷ Agrippa d'Aubigné, *Histoire universelle* (Paris, 1616–20), ed. Alphonse de Ruble (Paris, 1886–1909), 1: 227.

²⁸ Crespin, *Histoire des martyrs*, 2: 381; and Crespin, *Histoire memorable de la persecution et saccagement du peuple de Merindole et Cabrieres . . .* ([Geneva], 1556); see also Isambert, *Recueil*, vol. 12, no. 316 (Nov. 8, 1540).

²⁹ Crespin, *Histoire des martyrs*, 2: 675.



Fig. 1. Execution of conspirators at Amboise, 1560. Contemporary engraving. Photograph from Jean Héritier, *Catherine de Medici*, tr. Charlotte Haldane (New York, 1963), facing p. 208.

"Having dipped his hands in the blood of his headless companions, he lifted them as high as possible to the heavens and cried, 'Here is the blood of your children, O Lord; for this you shall be avenged.'"³⁰ This scene, depicted in an often-reproduced contemporary engraving (see fig. 1), was another to treasure in that martyrological canon that Hotman and Crespin were in the process of compiling.

The villain of this piece, of course, was the cardinal of Lorraine. He was denounced a few weeks later by Hotman as the "Tiger of France" in a pamphlet that was in effect the *J'accuse* of the religious wars. The cardinal, supported by his brother, the duke of Guise, was the true conspirator, Hotman charged, the one responsible for spilling the blood of so many innocents. "If Caesar was killed trying to gain the sceptre justly," he asked, "can we permit you to live, who pretend to it unjustly?" It must be added that Hotman himself was responsible for the creation of at least one more martyr, for during the summer the printer of his *Tiger* was seized by the police, who were apparently chasing a murderer at the time. The printer, a certain Martin Lhommet, was executed a month later in the Place Maubert near the

³⁰ [Hotman], *L'Histoire du tumulte d'Amboyse* (Strasbourg, 1560), BN, Lb.³² .15; see also [Hotman], *Vita Colinii*, 33.

university. Only three copies of Hotman's book seem to have survived: one in Strasbourg, one in Switzerland, and the other in Paris, which was to be discovered in the nineteenth century, reprinted, and made the center of an extensive controversy over the "freedom of the press."³¹

The next two years were a time of indecision, wavering duplicity, and futile attempts at compromise. Persecutions continued, especially against the Waldensians, and a recent historian has spoken with justice of "the impossible toleration of the Colloquy of Poissy" in the fall of 1561.³² Few hoped to prevent war; it was only a question of time, and the time came the following spring. The spark was provided by the tragic confrontation at Vassy between a force led by the duke of Guise and a Huguenot congregation—the "Sarajevo of the religious wars," as it has been called.³³ As Guise and his men approached, perhaps looking for trouble, they heard singing from the Protestant church—"I am afflicted and ready to die" is one of the verses from the psalm—and were outraged at this violation. Whichever side cast the first stone, the result was seventy-four Huguenots dead or dying and a political situation out of control. "If you will forgive a snap judgment," wrote one observer, "this is the beginning of a tragedy that we shall all be playing."³⁴ No Protestant hesitated to regard the affair as a full-fledged massacre. So within a few weeks Hotman and other observers represented it, and so later it would be interpreted by Huguenot historians, again including Hotman as well as Crespin.

The blood of these martyrs rapidly nourished the seeds of civil war. Barely a month later Huguenot forces gathered at Orleans and prepared for a major conflict. Hotman was among them and made the connection most explicitly. "In this event," he remarked of the affair of Vassy, "our leaders see the signal for a general massacre being prepared by our enemies in all parts of the kingdom."³⁵ Underlying this view one may see not only a kind of conspiracy theory of history, which was to become increasingly common, but also a doctrine of political expediency, which was to become a central feature of Huguenot propaganda and self-justification.³⁶ In either case the Massacre of Vassy was transformed into a myth—not a "nonevent" but a kind of "hyper-event," which served at once as an excuse for resist-

³¹ [Hotman], *Epistre envoyée au tigre de la France* (Strasbourg, 1560), ed. Charles Reade, *Le Tigre de 1560* (Paris, 1875), and the offprints among Reade's papers located in Paris, Bibliothèque de la Société de l'Histoire du Protestantisme Français, MS 816, vol. 4; see also *Calendar of State Papers, Venetian*, 8: 234.

³² Alain Dufour, "L'impossible tolérance au Colloque de Poissy," *Musées de Genève*, n.s. 4 (1963): 8–11.

³³ H. O. Evennett, *The Cardinal of Lorraine and the Council of Trent* (Cambridge, 1930), 21.

³⁴ Etienne Pasquier, *Les Lettres* (Paris, 1617), bk. 4, no. 15; other accounts of the massacre are in Crespin and Goulart, *Histoire des martyrs*, 2: 209; and *Memoires de Condé* (London, 1743–45), 3: 111.

³⁵ Hotman to Bonifacius Amerbach, Apr. 12, 1562, Universitätsbibliothek, Basel, MS G. II. 19, fol. 148r.

³⁶ See, e.g., *Advertissement sur la fausseté de plusieurs mensonges semez par les rebelles* (Paris, 1562), in Lindsay and Neu, *French Political Pamphlets*, no. 258, which is a Catholic response to the *Histoire comprenant en brief ce qui est advenu depuis le partement des sieurs de Guise* . . .

ance, as a means of shifting all guilt to the Guise party, and as a most satisfactory explanation for the coming of the wars of religion.

There were other "massacres" over the next decade, of course, but Vassy remained the archetype; and for the rest of the century Huguenots operated upon the assumptions generated—or rather confirmed—by this tragedy. The Massacre at Vassy fitted in perfectly with the belief, based upon countless statements of official policy, that the government was literally intent upon the "extermination" of those of the religion. The theme recurred again and again over the next decade. The most famous illustration is the meeting in 1565 between Catherine de' Medici and the duke of Alba. Although their conversations were generally inconclusive, they immediately provoked suspicions among Huguenots about a universal Catholic conspiracy, turning upon a Paris-Madrid axis linked in turn with the Council of Trent, whose canons and decrees had been published only the year before. This meeting between Catherine and the duke of Alba has always been associated with St. Bartholomew seven years later, but it should be understood that the legend surrounding it came earlier and was in fact part of the general fear of that Florentine woman, Catherine de' Medici. Hotman later published a letter by her, supposedly written in 1569 and intercepted; and although it is undoubtedly spurious, it sounded quite convincing to her Protestant critics. "To restore the crown of France," she is supposed to have written, "there is no better way than to kill all the Huguenots."³⁷

It was at this same time, that is, during the third war, that Admiral Gaspard de Coligny emerged as the leader of the Huguenots and as a figure of international influence. Protestants began looking to him as their savior, and indeed a number of cities and congregations placed themselves under his protection. In September he was officially condemned by the Parlement and deprived of his offices. According to this *arrêt*, which was printed in eight languages and widely distributed, Coligny was "giltie of traison, distourber and breaker of peace, ennemy of repos, and tranquillitie of the commonwealth: the Captain, author, and ringleder of the rebellion, conspiracie, and faction that hath bin made against the King and his State." It is very interesting to note that when Coligny perished in the Massacre of St. Bartholomew three years later, this same charge was resurrected and in fact the *arrêt* itself republished.³⁸

(Orleans, 1562), BN, Lb.³³ 48; see also *Bref discours et veritable des principales coniurations de ceux de la maison de Guyse . . .* (Paris, 1565), in Lindsay and Neu, *French Political Pamphlets*, no. 407, one of the many pamphlets carrying on the attack inaugurated by Hotman's *Tigre*.

³⁷ Quoted in [Hotman], *Vita Colinii*, 57. On "le desseing de Bayonne" see, e.g., the Huguenot pamphlets collected as *Les Requests, Protestations, remonstrances et advertissements, faits par Monseigneur le Prince de Condé . . .* (Orleans, 1567), BN, Lb.³³ 206; and *Discours au vray des conseils et moyens qu'on a tenus pour exterminer la pure doctrine . . .* (Heidelberg, 1568), BN, Lb.³³ .195.

³⁸ *Arrest de la Court de Parlement contre Gaspart de Colligny, qui fut admiral de France, mis en huit langues, a sçavoir, François, Latin, Italien, Espagnol, Allemand, Flament, Anglois et Escoçois* (Paris, 1569), republished (Paris, 1574), in Lindsay and Neu, *French Political Pamphlets*, nos. 652, 653, 754.

In these same years there was a growing tendency among Protestants to attach these fears and threats to malicious foreign, especially Italian, influence. Among other suspected imports during the early stages of the religious wars came "Papemania" and the practice of assassination—murder "Italian style," as Hotman's friend Henri Estienne called it—which was another source of martyrs.³⁹ Nor was the overall apprehension in any way allayed by specific pacification agreements, which on the contrary were suspected of being duplicitous arrangements made to trick Huguenots into letting down their guard. This was especially the case with the "limping and uneasy peace" of Saint-Germain of 1570, which Huguenots would later condemn as an evil trick and, according to one of Hotman's imaginative friends, as the diseased offspring of Catherine and her alleged consort, that devil of a cardinal (of Lorraine), who was accumulating his own share of "legends."⁴⁰

All of these proliferating and intertwining anti-Catholic legends tended, as legends will do, to group themselves together around a more visible and concrete symbol that could serve as the center of a grander construct. The symbol that finally emerged to take this sovereign position was the figure, or at least the public image, of Catherine's countryman Machiavelli, who came to represent a kind of devilish counterpart to the Protestant martyr ideal. Machiavelli could not assume this posthumous role, however, until there was a crime worthy of his evil genius; and such a "crime" was indeed provided on August 24, 1572. Taken together, this massacre, the Protestant conspiracy theory, and the patterns of martyrdom created a mythology of monumental proportions.

IT IS IN SUCH a conceptual and emotional context, it seems to me, that the events surrounding and succeeding St. Bartholomew's Day 1572 must be understood; and so must the leading actors in the drama. The Huguenot chief Coligny was the very prototype of the Protestant saint, as represented in the official, and indeed hagiographical, account written by Hotman.⁴¹ Coligny's days, even his meals, were filled with prayers, sermons, and psalm singing, and he was most solicitous about spreading the word through education and missionary work as well as by personal example. Yet he never ceased being a fighter, and in the name of these very ideals he had been playing a most dangerous political game, purportedly at the expense of that

³⁹ Henri Estienne, *Apologie pour Hérodote*, ed. Paul Ristelhuber (Paris, 1879), 1: 353; and *La Papemanie de France* (n.p., 1567), Arsenal, 8°H. 12774, vol. 1; see also Rabelais, *Pantagruel*, bk. 4, ch. 48.

⁴⁰ Anonymous poem, "La Paix Valois," in *Le Reveille-matin des François* ("Edinburgh" [Basel?], 1574); see also *Legende de Charles Cardinal de Lorraine*, vol. 6 of *Memoires de Condé*. In general, see my "Murd'rous Machiavel in France," *Political Science Quarterly*, 85 (1970): 545–59; and Salvo Mastellone, *Venalità e Machiavellismo in Francia (1572–1610)* (Florence, 1972).

⁴¹ [Hotman], *Vita Colinii*, 130; see also Crespin and Goulart, *Histoire des martyrs*, 3: 663.

Tridentine "conspiracy," which by now had taken on the appearance of a Madrid-Rome axis. Indeed he was doing a bit of conspiring himself, though apparently not without royal permission, and had committed himself to supporting William of Orange's projected invasion of the Netherlands. But in July Coligny's credit had fallen off sharply when a force of Huguenot troops, sent to Mons to relieve William's lieutenant Louis of Nassau, was ambushed and slaughtered by the Spanish.

Meanwhile in Paris tensions were mounting. The scheduled wedding of Henry of Navarre and Catherine's daughter Marguerite, a kind of nuptial prefiguring of the conciliatory policy later adopted by the Politiques, was a hopeful sign; but it had to be postponed because of the death of Henry's mother, Jeanne d'Albret, in June. Stories immediately began to circulate, and to be believed by Huguenots such as Hotman, that she had been poisoned by the enemy. In such a heated atmosphere the wedding was set for August 18. Alarming rumors continued to fly, and in July Charles IX issued another in a long line of ordinances banning weapons and expelling vagabonds and *mauvais garçons* within twenty-four hours. Just before the wedding, it seems, Jean de Montluc, a moderate Catholic friend of the Huguenots and a diplomatic agent for Catherine, warned one of Coligny's men to get out of the city. This Coligny himself could not do, and he took up residence in a house belonging, ominously enough, to the family of Anne du Bourg. "I would rather be with you than at court," he wrote to his young and pregnant wife a few hours after the ceremony, "but I must set public advantage above private pleasure."⁴²

The first act of the tragedy came on Friday of the same week. On that morning of August 22, as he was returning from court, Coligny was shot and seriously wounded by a certain Maurevert, called the "king's killer" because of a more successful attempt made against one of Coligny's lieutenants a few years before. The next two days Coligny spent in his quarters, attended by friends and his physician, Ambrose Paré, and visited by embarrassed members of the royal family. Coligny spoke at length and in saintly tones, providing political advice for the king, a confession of faith for his followers, and forgiveness for his would-be assassin, if not for the duke of Guise, whom he held responsible. Already, according to Hotman's secondhand account, Coligny was beginning to sound like a martyr. A royal guard was given to him but was placed under the command of an old enemy. The ban on arms was still in effect, but it did not apply to the king's men, who were rattling their weapons ostentatiously in the streets. So, as

⁴² Quoted in [Hotman], *Vita Colinii*, 105, but differing significantly from the extant original (which has been published in the *Bulletin de la Société de l'histoire du protestantisme Français*, 1 [1852]: 369) by adding a statement to the effect that the admiral would take care not to offend his enemies (presumably to offset the charges of hostility and militancy); see also *Ordonnance du Roy portant injonction a tous ses subjects de vivre en amitie les uns avec les autres* (Paris, 1572), Arsenal, 8°H. 12778, vol. 3.

Huguenots looked back on the situation, the trap had been set; indeed the events of that weekend all seemed to fall into one terrible pattern.

The second and successful attempt on the admiral early in the morning of August 24 set off the mythopoeic process. "I am ready for death," the admiral had said before the assassin struck, and soon the "Matins of Paris" began. "Away with him, cut of his head and handes,/ And send them as a present to the Pope," the duke of Guise has been represented as saying. The words are Christopher Marlowe's,⁴³ but the deed itself has been documented and has taken its place alongside of many other atrocity stories; babies dropped from windows, bodies stripped and thrown into the Seine, some to be seen for days afterwards, a few as far as Rouen, according to a Huguenot song. As usual Paris set the fashion for the provinces, and the attacks signaled by Coligny's murder, carried out by laborers as well as by gentlemen, spread to a dozen or so towns throughout France. Many Protestants fled into the countryside. "I am sure that the wild beasts are kinder than those in human form," Hotman remarked to a friend. Later he reported that "Huguenot-hunting"—*la chasse des huguenots*—was becoming a popular sport.⁴⁴

In the international Protestant community the reactions included shock and outrage but little real surprise. The inhabitants of La Rochelle had been warning Coligny of some such plot for two months, Hotman had heard, and throughout France men had been saying that Coligny was deceived at court.⁴⁵ "What an atrocity!" was Beza's reaction to the admiral's death. "How many times have I predicted this! How many times did I warn him about it!"⁴⁶ Streams of refugees carried the news in greater detail, but there seems to be little doubt about the underlying cause of the massacres. The plot was "undoubtedly general and the work of the Council of Trent," declared the Council of Geneva, which indeed feared that it would extend into their own territory.⁴⁷ During the fall one French agent confessed under torture that the invasion would be launched from Savoy, and though the attack did not materialize, the fear continued for the rest of the century. Hotman, then exiled in Geneva, shared and broadcast this apprehension about what he called "the Tridentine web and popish alliance called the Holy League."⁴⁸

⁴³ Christopher Marlowe, *The Massacre at Paris*, based on Hotman's well-known *De Furoribus Gallicis*; see also Paul Kocher, "François Hotman and Marlowe's *The Massacre at Paris*," *PMLA*, 56 (1941): 349-68.

⁴⁴ Hotman to Rodolphe Gualter, Nov. 5, 1573, Zentralbibliothek, Zurich, MS F. 39, fol. 214v; the same phrase is used by Nicolas Pithou, *Histoire ecclesiastique de l'église pretendue reformee de la ville de Troyes*, published by Charles Recordon as *Le Protestantisme en Champagne* (Paris, 1863), 144.

⁴⁵ Hotman to William of Hesse, Oct. 6, 1572, published in Ludwig Ehinger, *Franz Hotmann, Beiträge zur vaterländischen Geschichte*, vol. 14 (Basel, 1896), no. 24.

⁴⁶ Beza to Thomas Tilius, Sept. 10, 1572, published in *Bulletin de la Société de l'histoire du protestantisme français*, 6 (1858): 16.

⁴⁷ Registres du Conseil, Sept. 4, 1572, Archives d'état, Geneva, vol. 67, fol. 201r.

⁴⁸ Hotman to Bullinger, Oct. 25, 1572, Zentralbibliothek, Zurich, MS S. 127, fol. 95r.

One conspicuous sign of the meta-historical impact of the massacre was the extremism of the reactions to it from all quarters. The Huguenots, of course, screamed bloody murder. The leaders were all "assassins," Hotman proclaimed, "the likes of whom no age has tolerated."⁴⁹ And this specifically included the king, by whose "express command" the deed was admittedly done. On the other side Catholics, arguing that the attacks were designed to crush a Huguenot "conspiracy" arising from the "Theodor-Bezian infection," concluded that it was simply retribution. "There is no gallows, cross, or torture severe enough to punish the crime of a traitor or rebel," one royalist wrote.⁵⁰ Others celebrated the event as providential. So François de Belleforest, later one of Hotman's chief critics, declared it to be no less than a miracle; and he found this judgment to be confirmed by the appearance of the great nova (the first in modern history) of November 1572. "I know the heretics will laugh and tax me with superstition," he added.⁵¹ In fact the "heretics" were not laughing and had an explanation of the phenomenon hardly less "superstitious": to them it was the Star of Bethlehem returned, and it signified the salvation to come.

Quantitative estimates were likewise inflated. The massacre was so enormous, said one commentator, "that I doubt if posterity will ever believe it."⁵² And in fact posterity has not believed the figures given by contemporary critics. At first Beza himself cried that over 300,000 of his brothers had been killed.⁵³ Later estimates were commonly placed at 100,000 deaths and revised downwards to 50,000. A decade later a certain "N. Froumenteau" published what purported to be a statistical survey of the costs, social as well as economic, of the religious wars, and his figures offer evidence of the depth of contemporary feeling if not of exact totals. By that date, 1583, Froumenteau estimated that about 765,000 persons had perished in the wars; 76,010 were civilian casualties, and of these 36,000 could be classified as "massacred"; 4,500 bodies had passed Paris on the Seine, while 6,000 had been carried by the Loire. In addition he estimated that 12,000 women and girls had been raped, and he went on to remark that, since this sort of thing so often went unreported, the total was probably twice as great. If the war continued, he remarked, the total would indeed be 100,000 instead of only 36,000.⁵⁴

⁴⁹ Hotman to Abraham Sulzer, Oct. 3, 1572, *ibid.*, fol. 47r. See also *Declaration du roy de la cause et occasion de la mort de l'admiral* (Paris, 1572), in Lindsay and Neu, *French Political Pamphlets*, no. 729.

⁵⁰ Artus Desire, *La Singerie des Huguenots* . . . (Paris, 1574), 22; and *Discours de la mort et execution de Gabriel comte de Montgomery* (Paris, 1574), fol. 2v, in Lindsay and Neu, *French Political Pamphlets*, no. 790.

⁵¹ François de Belleforest, *Discours sur l'heur des presages advenus de nostre temps significantz la felicité de regne de nostre Roy Charles* (Paris, Nov. 18, 1572), fol. 10r.

⁵² *Resolution claire et facile sur la question . . . de la prise d'armes par les inférieurs* (Reims, 1577), 97, in BN, Lb.³⁴ .103.

⁵³ Beza to Bullinger, Sept. 1, 1572, Zentralbibliothek, Zurich, A. 44, p. 679, cited by P. F. Geisendorf, *Théodore de Bèze* (Geneva, 1949), 306.

⁵⁴ The book in question is the fascinating *Secret des finances de France* (n.p., 1581), dedicated to Henry III, by N. Froumenteau (no doubt a pseudonym), a friend of Hotman and author also

Whoever he was, Froumentau was a Huguenot and had nothing but contempt for the "Machiavellistes" who had so ravaged France. Yet, though his statistics are inflated by partisanship, it should be added that he did make some effort to indicate his sources (*preuves*) and often omitted provincial figures for lack of evidence, so that his conclusions are relatively far closer to reality than those of most chroniclers.⁵⁵ While it would be an error to take his figures at face value (as certain nineteenth-century scholars have done), it would be equally mistaken to deny this pioneering work a significant place in the history of statistics, which like other social sciences emerged at least in part from (and has never quite disassociated itself from) ideological conflict. In any case a great effort went into the making of the book, and it testifies again to the enormous intellectual impact of the wars of religion and of St. Bartholomew in particular.

In the field of political thought, of course, the impact of St. Bartholomew was even more spectacular. The massacre immediately took its place as the pivotal event in the martyrological tradition and became a central force in the flood of propaganda that poured from Protestant presses at a greater than average rate in succeeding years. It was at this point that the enlarged community of martyrs began to take on a more special political significance because of its association with the opposition party led by Henry of Navarre, who succeeded Coligny as the leader of the Huguenots. The point was made best by Beza in his *Right of Magistrates*, a tract so radical that he was refused permission to publish it in Geneva. "And this I conclude," he wrote, "that we must honor as martyrs not only those who have conquered without resistance, and by patience only, against tyrants who have persecuted the truth, but those also who, authorized by law and by competent authorities, devoted their strength to the defense of the true religion."⁵⁶ Here is expressed the complete politicization not only of the Calvinist cause

of the anonymous *Miroir des François* . . . (Paris, 1581), *Le Cabinet du roy* . . . (Paris, 1581), and, if only in part, of the *Reveille-matin*, which is one work that estimates the massacred of St. Bartholomew as 100,000 (p. 78). The first book includes a dialogue between "Provence" and "Le Politique" (who figures also in the *Reveille-matin* and the *Miroir des François*) and a listing, province by province, of the taxes, charges, and expenditures, especially military, over the previous thirty-one years. The second book discusses the social costs, again province by province (pp. 378-79). The categories of the dead are ecclesiastics, nobles (Catholics and Huguenots distinguished), soldiers (again Catholics and Huguenots), those executed (many for lese majesty), Huguenots "massacred," foreigners, houses destroyed, villages burned and razed, and *filles violees*. Froumentau's *Preuves* (pp. 401-09) include records of the *Chambre des Comptes* and *contrôles de la gendarmerie* as well as "chronicles and memoirs," but he adds that no "proof" is needed to describe the horror of the massacres.

⁵⁵ Froumentau, *Le Secret des finances*, 418. The work has not received its due in the history either of economic thought or of statistics; the judgment of Fernand Faure, "France," in John Koren, ed., *The History of Statistics* (New York, 1918), 236-37, contradicting such credulous nineteenth-century opinions as that of Henri Baudrillart, is probably excessively severe and certainly unhistorical.

⁵⁶ Beza, *Du Droit des magistrats* (Geneva, 1574), critical edition by R. M. Kingdon (Geneva, 1970), 67, and translated in Julian Franklin, ed., *Constitutionalism and Resistance in the Sixteenth Century* (New York, 1969), 135.

but also of the tradition of martyrs—an unmistakable shift from passive to active resistance.

Another conspicuous by-product of St. Bartholomew was the mythology, or demonology, associated with Machiavelli, whose ideas of political behavior were widely regarded as the cause both of the massacre itself and of the political and social degeneration of France in general over the previous ten years and more. Much of this material, especially the *Reveille-Matin* and Hotman's *French Fury*, consisted of highly colored accounts of the event itself or of extravagant elegies of the "hero-martyr" Coligny,⁵⁷ but there was also a growing quantity of polemic and theorizing about the problem of war guilt (which has always played so fundamental a role in historical thinking) and about ideas of political resistance, constitutional government, sovereignty, and the structure of society in general. Not only the work of the Monarchomachs but also Jean Bodin's *Republic*, which may be taken as a response to the work of the Monarchomachs, grappled quite directly with the problems that the Massacre of St. Bartholomew defined in a way that made them impossible any longer to ignore.⁵⁸

In more concretely historical terms the effect of the massacre was traumatic. Anticipated for a decade, after its coming it haunted an entire generation of Protestants, who suspected that it might be revived at any time and who were prepared, ideologically if not emotionally, to take their place among the future martyrs. Hotman, for example, having been sent into permanent exile, lived out his remaining eighteen years in almost perpetual fear that the fate he had just barely missed in 1572, and that had claimed so many of his friends, including Petrus Ramus, would overtake him. The tone of his correspondence is consistently that of a victim and even martyr. At one point he was convinced that the pope had hired a man to assassinate him, and in his last years he became obsessed with the idea of dying in battle for his Cause—one of the established "marks of martyrdom." Only the thought

⁵⁷ See, for example, *Epicedia illustri heroi Caspary Colignio . . . Beato Christi martyri, variis linguis a doctis pijsq. poetis decantata* (n.p., 1572), in Lindsay and Neu, *French Political Pamphlets*, no. 725, of which Beza's own presentation copy is in the possession of Yale University Library. Professor Samuel Kinser of Northern Illinois University points out a most interesting example of the hero-martyr dichotomy in d'Aubigné's account of Coligny's death. Following the account of Jacques-Auguste de Thou, *Historiarum sui temporis opera* (Frankfurt, 1614-21), 1: 993, and of [Hotman], *Vita Colinii* and, with some modifications [Hotman], *De Furoribus Gallicis*, d'Aubigné first represented the admiral, on the point of death, as adopting a hero's stance and lamenting the fact that he was about to be killed at the hands not of a "cavalier" but only of a common servant. But a few years later, in the second edition (1626), d'Aubigné changed the picture so that Coligny appeared as a passive victim, "on his knees beside his bed" and making only the martyrlly comment, "My friends, save yourselves." See *Histoire universelle*, 3: 313.

⁵⁸ For discussions of this large subject, see Ralph Giesey, "The Monarchomach Triumvirs: Hotman, Beza and Moruay," *Bibliothèque d'Humanisme et Renaissance*, 32 (1970): 41-56; and J. H. M. Salmon, "Bodin and the Monarchomachs," in a forthcoming volume based upon the international conference on Jean Bodin held in Munich in 1970. It has recently been argued that the monarchomach author of the *Vindiciae contra tyrannos* (n.p., 1578) was not Philippe du Plessis Mornay but rather Johan Junius de Jonge: see Derek Visser, "Junius: The Author of the *Vindiciae contra Tyrannos*?" *Tijdschrift voor Geschiedenis*, 84 (1971): 510-25.

of his children, he said, restrained him.⁵⁹ In many ways his attitude was typical of the French exile community of his generation, whose politics, perspective, and very lives had been shaped by the massacre.

Situated as it was at the center of such swirling emotions, revolutionary implications, festering resentments, and indeterminate intellectual repercussions, the Massacre of St. Bartholomew became a legend almost before it happened, and it grew with the telling and with the passage of time. As it furnished a target and motive for endless polemic, so it furnished a target and motive for scholarly debate—a classic “problem” for historians, though ultimately insoluble, at least in the guild-oriented, legend-prone, history-transcending terms of sixteenth-century propaganda, from which we have hardly yet escaped.⁶⁰ “The blood of martyrs is the seed of the church”: it has also been fuel for the labors of generations of historians, and by now more ink than blood has flowed as a result of the events of that weekend in Paris four hundred years ago. Catholic masses have ceased, Protestant lamentations have been muted, but the historical discussion continues: such is one of the forms, it seems, that myths take in our time.

⁵⁹ Hotman to Simon Grynaeus, Nov. 27, 1586, in B. F. Hummel, ed., *Celebrium virorum . . . epistolae ineditae* (Nuremberg, 1777), 81; and Hotman to Daniel Tossanus, Feb. 26, 1588, in *Hotomanorum epistolae* (Amsterdam, 1700), no. 162, citing *Aeneid*, bk. 2, line 353.

⁶⁰ Herbert Butterfield, “Lord Acton and the Massacre of St. Bartholomew,” in his *Man on His Past* (Cambridge, 1955), 171–201, is a useful survey.

“The Sense of an Impending Clash”:
English Working-Class Unrest before the First World War

STANDISH MEACHAM

HISTORIANS WHO SET out to understand early twentieth-century England must sooner or later come to terms with George Dangerfield's *Strange Death of Liberal England*.¹ An elaborately extended metaphor, the book describes a “liberal” England that is part Gladstone and part Rupert Brooke, apparently ageless, yet doomed because unable to practice any longer the deceiving homilies it has preached for over half a century: individualism and economic servitude; self-help and deference; democracy and class-consciousness. Its death signals the birth of a new and very different England, one of strikes, suffragettes, and turmoil over Ireland, a nation as inclined to balk at compromise as was its predecessor to indulge in it.

Dangerfield appears ready to welcome this new, post-liberal England, insisting as he does that the conflicts of 1911–14, far from signs of decadence, were evidence of rebirth. Yet his dazzling impressionism is not wholly convincing. To describe Georgian England as casting off the traditions and institutions of the past implies a break with the Victorian age that had by no means come by 1914. It fails to account for the traumas of the ensuing fifty years, heralded by Strachey's *Eminent Victorians* and marked out by Versailles and Suez, in which the English have had to face down a far from moribund nineteenth century.

A second implication appears more plausible. At the root of Dangerfield's thesis lies his belief that Georgian unrest was all of a piece. Suffragettes, Orangemen, and syndicalists are joined together in one pattern to describe his “new” England. Most historians have agreed. They speak of a “mood” that, according to their predilections, may or may not signal the end of Victorian liberalism, but that almost invariably combines labor unrest, feminist militancy, and Irish insurrection into one general prewar cataclysm. Henry Pelling has now challenged that notion. In a chapter in his recent *Popular Politics and Society in Late Victorian*

¹ George Dangerfield, *The Strange Death of Liberal England* (New York, 1961; 1st ed., 1935).

Britain,² he chides historians generally and Dangerfield particularly for a readiness to see a connection where none exists. Pelling is a no-nonsense historian. For him one hard fact is worth a dozen impressionistic hypotheses. He takes actions more seriously than words. His judgments are commensurately careful and refined. He refuses to see labor unrest as part of a general protest against nineteenth-century liberalism or parliamentarianism, concluding simply that it "was only coincidental with the acute phases of the Irish and women's suffrage questions. It had its own independent and sufficient causes": primarily a low unemployment rate, which encouraged union growth, and the introduction of national insurance, which threatened union organization. "In any case, it owed little to feelings of disappointment with parliamentary institutions or existing political parties."³

Correct or not, Pelling's cold douche should compel scholars to re-think their conclusions about this complicated period. Their compulsion to do so will be fortified by their conviction that a historian's particular task is the tracing and untangling of connections. No era of English history presents a more bizarre and challenging puzzle than this one. If Dangerfield has imposed an over-elaborate design upon the facts, he has given us facts too compelling to put aside. These accounts of English men and women up in arms, dismayed and occasionally furious without knowing quite why, strike us with an immediacy—I deliberately avoid "relevancy"—born of our own more recent experiences, making us all the more anxious to understand the fiercely articulated aspirations and frustrations of another period in time.

To do so we shall begin, as Pelling does, with the labor unrest and, again as he does, with some facts. His statistics show that union membership during the years 1911–14 expanded markedly, with 1911 and 1913 experiencing the greatest percentage of growth (22 per cent and 21 per cent respectively).⁴ Total membership in all unions rose by over 60 per cent from 1910 to 1914; membership in the so-called "new unions"—dockers, seamen, and general laborers—increased by over 300 per cent. The spread of strike activity in the 1911–14 period was just as marked. From an average of 480 strikes per year for the years 1907 to 1910, the figures leap upward: 872 in 1911, 834 in 1912, 1459 in 1913, and 972 in 1914 (a figure that would have been much higher had not England gone to war in August). Figures for the total number of working days lost are more erratic, though they are generally higher from 1911 to 1914 than from 1907 to 1910.⁵

² Henry Pelling, "The Labour Unrest, 1911–1914," in his *Popular Politics and Society in Late Victorian Britain* (London, 1968), 147–64.

³ *Ibid.*, 164.

⁴ These and subsequent figures from the table in *ibid.*, 149.

⁵ Figures in thousands: 2,150 (1907); 10,790 (1908); 2,690 (1909); 9,870 (1910); 18,169 (1911);

These are clearly years of intense labor activity and unrest. By linking them to earlier periods of trade-union growth Pelling hopes to temper the impulse to treat the events of 1911–14 as unique. Although he credits syndicalism with an appeal among younger trade unionists and with some measured success within the railway unions, he will have nothing to do with the notion—suggested by Elie Halévy and subsequently elaborated by Dangerfield—that the age was one of syndicalist revolt. His caution is justified. Halévy himself notes that “during those very years in which revolutionary syndicalism was so vocal, cooperation between the trade unions and the Government became closer than before.”⁶ Dangerfield contents himself with calling syndicalism “a convenient expression for a new energy.”⁷ The expression was Continental in accent, if not in intent. Tom Mann, who was not embarrassed to call himself a revolutionary, preached “control of industry by ‘syndicates’ or Unions of workers, in the interest of the entire community.” Revolutionary in aim, the syndicalist movement urged the abolition of the wage system; revolutionary in method, it spurned long-term contracts between workers and masters. Syndicalists talked of an end to parliamentary government. “We shall have no need to plead with the parliamentarians to be good enough to reduce hours as the workers have been doing for fully twenty years without result. We shall be able to do this ourselves, and there will be no power on earth to stop us so long as we do not fall foul of economic principles.”⁸

The pronouncement borrowed its theories and tone from Europe. But, as E. H. Phelps Brown remarks, the British strikers really had very little in common with those on the Continent who seized town halls.⁹ Lacking a revolutionary vocabulary of their own, English workers used syndicalists’ words to demand not *syndicates* but industrial unions and a more militant anticapitalism. Union members called for a minimum wage and for the Triple Alliance; at the same time they refused to forsake independent political action through the Labour party.¹⁰ They listened to Tom Mann’s rhetoric but did not subscribe to his doctrine. No doubt, as

40,890 (1912—the year of the national coal strike); 9,800 (1913); 9,880 (1914). These figures suggest comparisons with the findings of Edward Shorter and Charles Tilly. They conclude that as organizational techniques increase, along with union membership, a strike turns from a test of endurance into a show of strength. Strikes are shorter but more frequent. Statistics for England in this period reflect the same general trend. See “The Shape of Strikes in France,” *Comparative Studies in Society and History*, 13 (1971): 67.

⁶ Elie Halévy, *The Rule of Democracy*, 2 (London, 1952): 479.

⁷ Dangerfield, *Strange Death*, 233.

⁸ Tom Mann, *Memoirs* (London, 1967; 1st ed., 1923), 263, 206.

⁹ E. H. Phelps Brown, *The Growth of British Industrial Relations* (London, 1965), 336. Peter Stearns, in *Revolutionary Syndicalism and French Labor* (New Brunswick, 1971), argues convincingly that syndicalism made far less impact upon the French working class than had been previously supposed.

¹⁰ At the annual TUC meeting in 1912, delegates voted 1,693,000 to 48,000 for a resolution affirming support for independent political action and the “centralization of social and industrial questions in the hands of the Government and local authorities.” Speeches during the debate left no doubt that this was a test for or against syndicalism. B. C. Roberts, *The Trades Union Congress* (London, 1958), 253.

Pelling points out, the language of syndicalism appealed to those among the working class who had no sympathy for bureaucratic socialism of the Fabian stripe. But the response that appeal in turn engendered looked less like revolution than like consolidation for the purpose of more assertive and successful negotiation.¹¹

To say that the syndicalists made little real headway is not to say that the working class remained immune from the violent fever Dangerfield claimed to have diagnosed. Pelling, not surprisingly, makes little of such notions. He is seconded by others, for example B. C. Roberts, who remarks in his history of the Trades Union Congress that though the militant spirit of the workers influenced debates, "the weight of tradition, vested interest, and the common sense of the delegates proved too great to be easily pushed aside. Strong sentiments were expressed, but no revolutionary policy was adopted, and no new faith embraced."¹² Phelps Brown voices surprise that England and Europe did not experience more violence, in view of the sharp check on earning power the working class sustained during the prewar years.¹³

The surprise of those alive then, however, was at the extent and bitterness of the disorders. Sir George Askwith, dispatched in the summer of 1911 to mediate at Hull, reported the remark of a town councilor who had experienced Paris during the Commune "and had never seen anything like this: . . . he had not known there were such people in Hull—women with hair streaming and half nude, reeling through the streets, smashing and destroying."¹⁴ Those with a sense of the past might have derived consolation from the memory that working-class history in the nineteenth century was marked by violence: Captain Swing, plug plots, Sheffield outrages, Bloody Sunday. Yet the disturbances of 1911 and after seemed at the time, and contemporary descriptions make them seem now, to possess a quality of their own. Something—the ever-increasing numbers involved, the restless, never-ceasing pattern of agitations—fused them into an expression of mass dissatisfaction and mass uncertainty that was unprecedented and therefore alarming.

Nothing quite equaled the summer of 1911. Phelps Brown chronicles the events in a chapter appropriately entitled "Strife": The seamen's

¹¹ See Alan Bullock's remarks regarding a 1914 speech by Ernest Bevin in favor of federation. "It underlines the preoccupation with the practical problem of organization—unity to give 'not so much a power to attack as a power to negotiate . . . the most valuable thing we can have'—as distinct from those who put in the forefront the syndicalist argument in favor of trade-union unity, the general strike as a means to the revolutionary seizure of power." *The Life and Times of Ernest Bevin* (London, 1960), 1:41. Stearns suggests that syndicalism, as a doctrine, could appeal only to those who—unlike almost all trade unionists—were alienated from the industrial system. "In fact," he notes with regard to France, "syndicalism was largely irrelevant precisely because the workers capable of protest, led in fact by the most skilled, accepted the industrial system." *Revolutionary Syndicalism*, 106.

¹² Roberts, *Trades Union Congress*, 247.

¹³ Phelps Brown, *British Industrial Relations*, 336.

¹⁴ Lord Askwith, *Industrial Problems and Disputes* (London, 1920), 150.

strike a week before the coronation in June; a railwaymen's strike—pressed by the rank and file upon the leadership—in August. Rioters shot dead by troops in Liverpool; cavalry and infantry in the streets of London; the plundering of a train in Wales; Jews driven from their shops in Ebbw Vale.¹⁵ And in another part of the forest, Agadir, suffragettes in the Albert Hall, and Carson prophesying Ulster's armed rebellion. The crisis subsided, but not the mood. Ben Tillett, the dockers' leader, called for class war in his annual report of 1912.

The class war is the most brutal of wars and the most pitiless. The lesson is that, in future strikes, the strikers must protect against the use of arms, with arms; protect against shooting, with shooting; protest against violence, with violence. . . . The other lesson is that Parliament is a farce and a sham, the rich man's Duma, the employer's Tammany, the Thieves' kitchen and the working man's despot. . . . Capitalism is capitalism as a tiger is a tiger; and both are savage and pitiless towards the weak.¹⁶

Tillet was stung by the defeat his union had suffered when it failed to achieve a national stoppage in 1912. But his angry words bespeak a deeper and more widespread discontent. Evidence of it was the apparently endless, uncontrollable, predominantly unofficial series of strikes in 1913 and 1914—937 in the last six months before the outbreak of war. Unions pressed ahead with recruitment and amalgamation, the better to negotiate, but uncertain what they were negotiating for. Men and women reacted more often than not from instinct. The Triple Alliance between railwaymen, miners, and transport workers is a case in point. P. S. Bagwell, historian of the railwaymen, makes it clear that the leadership of the National Union of Railwaymen had no intention of exacerbating the class struggle “by using every strike in the coal mining, dock, and railway industries as an occasion to draft reinforcements to the strikers from the million-and-a-half trade unionists covered by the agreement.” Yet the rank and file expressed the belief that—in their words—the alliance would “encourage the growth of greater solidarity and a vast improvement in the social conditions of the workers, and be a powerful lever in the course of working-class emancipation.”¹⁷ As Phelps Brown remarks, the agreement laid the members “under no obligation to strike together, or indeed to do anything save to consult one another before proceeding with a major issue. . . . Nonetheless it was understood to have pledged all to back the cause of each. Nothing could have been less thought out.” How to explain such thoughtlessness? The generally cautious Phelps Brown begins to sound like Dangerfield.

¹⁵ Phelps Brown, *British Industrial Relations*, 321–23.

¹⁶ From the Dock, Wharf, Riverside and General Workers' Union Annual Report, quoted in Bullock, *Ernest Bevin*, 1:35.

¹⁷ P. S. Bagwell, *The Railwaymen* (London, 1963), 307.

That it should ever have been adopted can be explained only by the amount of unemployment among railwaymen and the transport workers that the coal strike had caused in 1912, prompting the thought that if all must be in it together in practice they might as well go in with a will; and by the sense of an impending general clash, a civil war between capital and labour, that was strong at that time of so many clashes.¹⁸

"The sense of an impending clash" is the sense of the evidence. It would appear to call for further accounting than that provided by Pelling. We can talk of trade-union growth and development, but we need to talk of something more—the sense that sniffed that clash between capital and labor. And as we know more of that sense, we shall be better prepared to judge if it shared in that more general mood of which so many historians have felt compelled to speak.

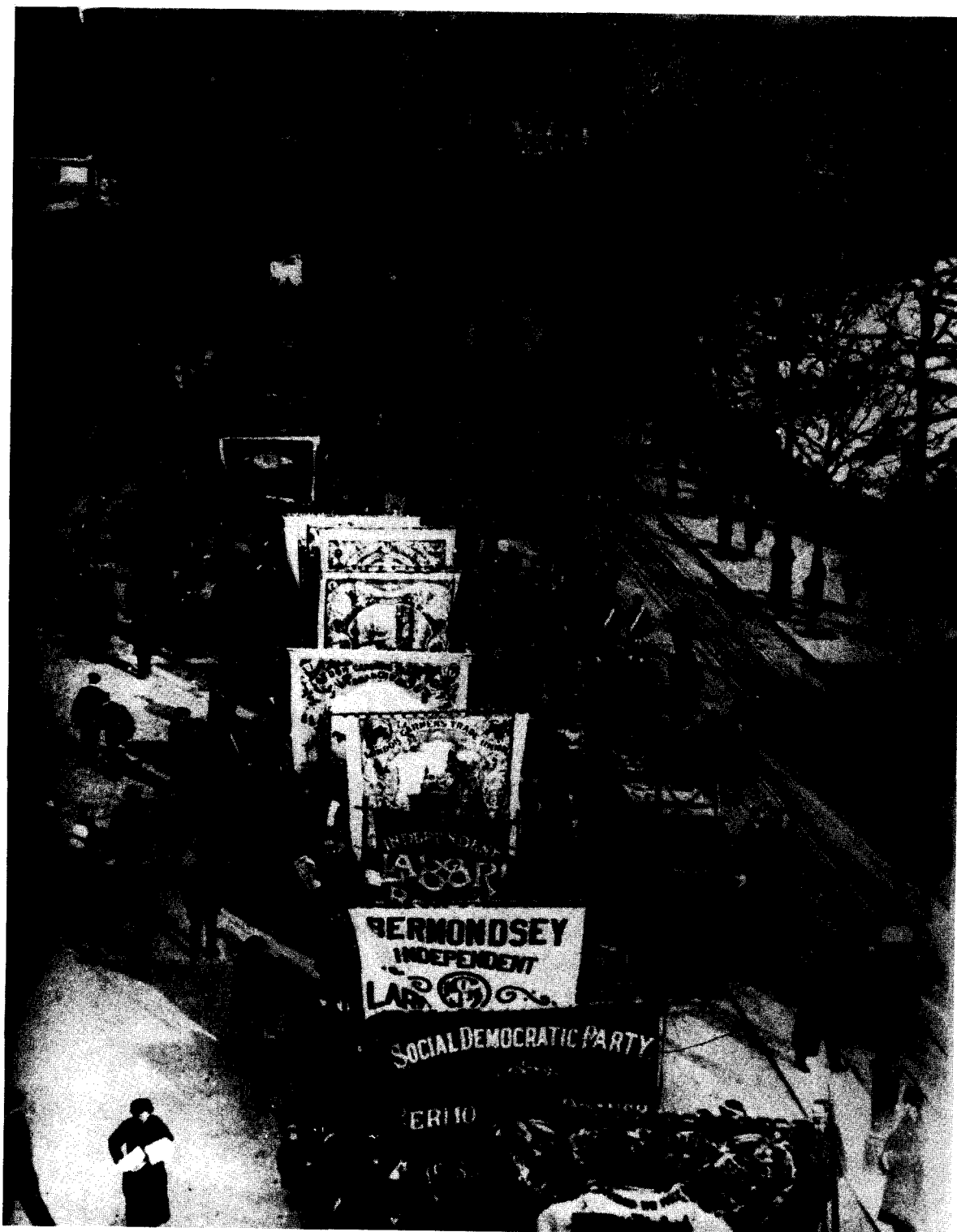
THE SENSE MAY well have sprung from economic circumstance. The general increase in real wages enjoyed by members of the English working class during the second half of the nineteenth century had induced them to take a constantly improving standard of living for granted. As working-class incomes rose,¹⁹ a significant drop in the cost of living and the importation of large quantities of cheap food combined to provide a decent life for at least some of those families upon whose labor England's industrial revolution had been built. Very suddenly, around 1900, the pattern changed. Real wages declined, production rates faltered, and Britain's balance of trade began to run against her. Prices rose steadily from the late 1890s until 1901 and again from 1907 to 1913. Total national consumption rose only three per cent faster than did population; consumption of food and clothing remained almost constant: a meager enough record under any circumstances but paltry in the extreme when compared with that of the previous thirty years. By 1914 real wages managed to catch up with the cost of living.²⁰ But the race was uneven and especially exasperating for the general worker, who, if he bothered himself at all with statistics, took little consolation from the fact that the worst was over, the worst having left him exhausted and a good deal poorer than he thought he ought to be.

Askwith, the Liberal party's peripatetic labor negotiator, did not hesitate to ascribe the workers' discontented mood to economic distress.

¹⁸ Phelps Brown, *British Industrial Relations*, 330.

¹⁹ Leone Levi, *Wages and Earnings of the Working Class* (London, 1885), 55. Levi estimates that from 1851 to 1881 working-class incomes rose 59 per cent. He estimates that incomes of the middle to upper classes had decreased by 30 per cent, and those of the lower middle class had increased by 37 per cent, during the same period.

²⁰ Phelps Brown, *British Industrial Relations*, 17. See pp. 13 ff. for a full and clear discussion of these factors. Using a 100 per cent figure as a base for 1880, Arthur L. Bowley estimated in 1920 that real wages increased to a figure of 132 per cent by 1900 but hovered between 132 and 134 per cent from 1900 to 1913. See *The Change in the Distribution of the National Income, 1880-1913* (Oxford, 1920), 18.



Trade-union demonstration at the Embankment, London, ca. 1902.
Photograph: Gernsheim Collection, University of Texas.

Trade had been improving, but employers thought too much of making up for some lean years in the past, and of making money, without sufficient regard to the importance of considering the position of their work-people at a time of improvement of trade. Prices had been rising, but no sufficient increase of wages and certainly no general increase, had followed the rise.²¹

The argument has continued to convince most historians, though few have troubled to discover the extent to which strikes were a conscious reaction to increased living costs. Strikes that workers may have considered aggressive—demands for more take-home pay—may in fact have been unconsciously defensive reactions to a decline in real wages, of which the workers themselves were only dimly aware. Pelling, who does not put much stock in the rising-cost-of-living argument, quotes K. G. J. C. Knowles to the effect that strikes more often occur during deflationary periods, when employees are faced with the tangible problem of a wage cut.²² Pelling suggests that the low unemployment figures for the years 1911 to 1914 (never higher than 3.3 per cent) explain the increased pressure of working-class demands. The masters needed the men, and the men took advantage of that need to press their claims. Pelling supports this thesis by pointing out that the two most recent periods of intensive union growth and agitation—1888–91 and 1896–1901—were also periods of full employment. Of course one can agree with Pelling without dismissing as irrelevant the factor of a decline in real wages. One can say simply that the related factors of a decline in real wages and the low rate of unemployment gave workers both their grievance and their power to articulate it.

As their demands accorded with their economic discontents, they refused any longer to countenance the nineteenth-century shibboleth that pegged wages to profits. "They held that the payment of a wage determined not by supply and demand but by human needs and common decency should be a first charge upon their industry."²³ To this extent they were pressing a new claim, one that Dangerfield's intuitive analysis describes well when he contrasts what he terms strikes about wages with strikes about money.

²¹ Askwith, *Industrial Problems and Disputes*, 175.

²² Pelling, "Labour Unrest," 150. See K. G. J. C. Knowles, *Strikes: A Study in Industrial Conflict* (Oxford, 1952), 223: "At the level of strikes, at any rate, Trade Unions seem to have reacted to the need for resisting a money wage decrease much more strongly than to the need for achieving a money wage increase to keep pace with the cost of living." Evidence that the workers were aware of an increasing cost of living appears in the *Annual Report* of the General Federation of Trades Unions for 1911, p. 6: "The case for increased wages was never stronger than it is today; trade is booming, profits are increasing, and wealth is accumulating, but wages remain almost stationary. Between 1900 and 1908 nominal wages have risen only one per cent; in London food prices have risen during the same period by nine per cent while profits have risen by twelve and a half per cent." Quoted in Alan Fox, *A History of the National Union of Boot and Shoe Operatives* (Oxford, 1958), 341.

²³ Phelps Brown, *British Industrial Relations*, 313.

While a strike about wages demands either a definite rise or a restoration of a definite cut, a strike about money comes from a sense of injustice. It is not specific, but incoherent and ominous. It is a voice in the wilderness, crying for recognition, for solidarity, for power. Its echoes are innumerable.²⁴

In some industries workers continued to strike for wages; in others, as Dangerfield suggests, they struck for money. Employers understood even less than workers just what was at stake. And since they settled strikes with wage increases, they failed, naturally enough, to understand why the workers remained unsatisfied.²⁵

There is a chance, then, that Phelps Brown's "sense of an impending clash" was rooted in something broader, if more ill-defined, than economic circumstances alone. Recognition, solidarity, and power—Dangerfield's ominous echoes—were union goals in the years 1911 to 1914 as they had been since the middle of the nineteenth century. But just as economic circumstances during those years heightened the workers' sense of economic injustice, social conditions induced them to proclaim in a more pronounced way their identity as members of a separate working class.

The twenty or thirty years before the First World War in England witnessed increasing class division. Enmity grew between employers and workmen. No longer did clever, ambitious artisans rise into the ranks of the entrepreneurial middle class, as they had with some regularity in the beginning and even the middle periods of the century. Nor could workers any longer persuade themselves of an identity of interest between capital and labor.²⁶ Factory masters gave way to directors, whose interest in their employees' welfare often amounted to indifference. Resentful of the boycotts and sympathy strikes of the "new" unionists after 1890, management obeyed the letter of the new Factory Acts, paid what the government forced it to pay in liability insurance claims, and threw itself with a will into the formation of a British Employers' Defense Union to counter union activity. Employers may have spoken with more wisdom than they knew when they observed, during negotiations with the Engineers in 1908, that demands such as the minimum wage were transforming "commercial problems into class problems."²⁷ If such was the

²⁴ Dangerfield, *Strange Death*, 249.

²⁵ See, in this connection, Phelps Brown, *British Industrial Relations*, 321. "What the men wanted [in 1913] was often hard to tell; to their employers it seemed the upheaval was simply revolutionary; but post by post and trade by trade, settlements were reached by raising wages."

²⁶ Among others, the miners had for a time argued this point in order to persuade the owners to curb outputs. "The miner would be rescued from the mire of poverty, obviously caused by too much coal on the market, and the coal-owners too would prosper exceedingly. What a striking proof it seemed to be of the identity of interest of Capital and Labour. If only the coal-owners and themselves could get together and agree to restrict production, the Law of Supply and Demand would do the rest." But once the miners began to win wage demands, they were quick to drop this argument. R. Page Arnot, *The Miners* (London, 1949), 1:125, 127.

²⁷ H. A. Clegg, Alan Fox, and A. F. Thompson, *A History of British Trade Unions, Since 1889* (Oxford, 1964), 1: 433. The period 1900-07 did see genuine attempts on the part of both labor and management to make the system of collective bargaining work. But the general picture remains one of increasing animosity. See *ibid.*, 362.

case, their actions as well as their employees' demands had made it so.

Sharper class distinctions manifested themselves outside the factory as well. The middle classes now spoke King's English, or at least tried very hard to; the working class did not. Middle-class parents had an average of four children; working-class parents, an average of six. In matters of pay, dress, drink—and drunkenness—health, life, and death, the classes appeared to be as far apart as they were in the 1840s and in some cases were further apart. The result was a heightened sense of class-consciousness, along with the decline and fall of that ideal which mid-Victorians had hoped might make of England one sturdy phalanx of bourgeois aspirations and sentiments. All classes now felt threatened and went on the defensive. The upper class foresaw ruin in Lloyd George's mad tax schemes; the middle class, in labor unrest and foreign competition; the working class, in a plutocracy that permitted everyone else to make money at the laborer's expense. They were at each other's throats, until one begins to suspect that only George Askwith, tirelessly and infinitely patient, could command attention and respect in every quarter.

That the working class listened to Askwith was a tribute to his personality, not his profession. He was a lawyer, at a time when laboring men had little use for the law. Their widespread mistrust of the law furthered their sense of a beleaguered "we" forced to take measures against an alien "they." Throughout the nineteenth century unions and workers had struggled for security and recognition within a hostile legal environment. Each advance appeared to produce a new set of obstacles. The Taff Vale decision of 1901, which held unions liable for damages incurred by individual members during a strike, and the Osborne judgment of 1909, which declared that unions could not levy dues for political purposes, were only the two most obnoxious in a long series of legal setbacks suffered in the years after 1890. *Temperton v. Russell* (1893) ruled against boycotts. *Trollope v. London Building Trades Federation* (1895) declared union officers who published blacklists of non-union firms and free laborers to be guilty of conspiracy. *Lyons v. Wilkins* (1899) pronounced against picketing, if it was determined to be "picketing to persuade"—an interpretation which inclined one judge to remark that "you cannot make a strike effective without doing more than what is lawful."²⁸ *Charnock v. Court* (1899) found two union men guilty of watching and besetting after attempting to persuade strikebreakers in Halifax to return to Ireland and offering to pay their fare.²⁹ *Quinn v. Leathen* (1901) overturned an earlier decision favoring the Boilermakers' attempt to establish a closed shop. All the opinions showed the

²⁸ *Ibid.*, 309. These decisions are treated in detail on pp. 308–11.

²⁹ "The effect was to render actionable any communication even of the most peaceable kind, except by letter, between strikers and men whom the employer was seeking to engage." *Ibid.*, 309.

courts in a mood to do everything in their power to curb unions and to prohibit strikes.

Hostile judgments were not the sole source of working-class disaffection for the law. Pelling takes Goldsmith's saw—"laws grind the poor and rich men rule the law"—as his text for a chapter in which he convincingly demonstrates that in many areas—debt, divorce, compensation, the selection of juries—the law conspired in explicit ways against the wage earner and the propertyless.³⁰ And with increasingly efficient law enforcement agencies, he points out, this law was increasingly brought to bear upon the resentful working class. What workers found most repellent was the obvious double standard. The Law Lords condemned the unions' boycotts and blacklists but sanctioned them when instigated by a ship-owners' cartel.³¹ The Osborne judgment placed obstacles in the path of union officers, but not in the way of railway company directors, who wished to sit in Parliament. Tom Mann was arrested for incitement to mutiny; Sir Edward Carson sat unmolested in the House of Commons. If Ulster was to be allowed to arm, asked J. H. Thomas, why should the unions not put their funds to use as well in preparation for the revolution? So bitter were workers at their treatment by the law that when it came time to draft a bill restoring the status quo ante Taff Vale, the unions insisted that they be allowed to remain immune from the law. They would not accept from the Liberals an act that appeared to leave their fate in the hands of the judiciary.

Resentment of the law not unnaturally grew, in the minds of some, into a resentment of parliamentary politics. G. D. H. Cole and Raymond Postgate oversimplify when they suggest, in *The Common People*, that the workers in these years deserted the Labour party for direct action.³² Once more Pelling provides the cautionary counter-evidence, balancing the party's electoral losses against its quite substantial organizational growth.³³ Yet some of the workers clearly felt a growing exasperation and impatience, a conviction that a Parliament in which, for example, over one-tenth of the members were railway directors really cared very little for the working class. John Hood, an outspoken employee of the Cambrian Railway, was fired after he testified before a parliamentary committee. Parliament admonished the railway but did nothing to get Hood his job back.³⁴ Mingled with the resentment was confusion as to

³⁰ Pelling, "Trade Unions, Workers and the Law," in *Popular Politics*, 62–81.

³¹ Roberts, *Trades Union Congress*, 173, n. 1.

³² G. D. H. Cole and Raymond Postgate, *The Common People* (London, 1938), 460.

³³ Pelling, "Labour and the Downfall of Liberalism," in *Popular Politics*, 117. Affiliated trade-union membership grew from 904,496 in 1906 to 1,572,391 in 1914. Affiliated trade councils and local branches of the Labour party rose in number during the same period from 73 to 177. The number of elected Labour party representatives on local governing bodies increased from 56 in 1907 to 184 in 1913. Pelling's source for these figures is the Labour party Annual Reports.

³⁴ Bagwell, *The Railwaymen*, 160–67.

the role the Labour party should play and the sort of legislation it should attempt to introduce. The election victory of 1906—even more the series of welfare measures passed in the ensuing years—appear to have taken many Labour politicians by surprise. Their campaign in 1906 had been fought on an essentially old-fashioned, radical platform: recognition of trade unions, elected school boards, free trade. Churchill and Lloyd George threw them off balance. Dished by the Liberal whizbangs, they wondered just what it was they had been elected to accomplish, and whether they were to continue to take their cue from the Liberal leadership. If that was the case, they asked themselves, how did they differ from those self-effacing Lib-Labs they had been sent to replace?

The uncertainty and frustration resulted in many cases in a general revulsion against Parliament, an institution, like the law, apparently designed by a ruling class for its own particular ends. “What is this great Parliament to whom we have entrusted our liberties composed of,” asked George Baker, a member of the Miners’ National Executive, during the coal strike of 1912.

It consists of six hundred and seventy men, six hundred and thirty who were capitalists and landowners, and it will be the death knell to the liberties of this movement if we hand them over to a body of this character, therefore I say we cannot hope to get much from those who represent those great interests of the country. I say that we must hold the sovereign power in our own hands.³⁵

Parliament provided valets to assist members into their dinner jackets, Fred Jowett complained, but not with clerks to help them draft legislation. “The Statute Book has been strewed by generations of lawyer members of Parliament with pitfalls for the unwary plain man, and unless you can afford to pay for expert assistance you must flounder as best you can.”³⁶ Jowett stood with Keir Hardie and Philip Snowden in denouncing the whole game of parliamentary politics. “All this jiggery-pokery of Party Government,” he told the ILP in 1909, “played like a game for ascendancy and power, is no use to us.”³⁷ The ways of Parliament were not the ways of the working class. Jowett proclaimed the conviction of many men and women, whose disaffection fed their class-consciousness, and that consciousness in turn helped fuel their “sense of an impending clash.”

A NOT UNREASONABLE hypothesis, then, would suggest that economic realities and a heightened sense of class combined to produce the severe labor unrest that occurred in England before the First World War. It

³⁵ R. Page Arnot, *The South Wales Miners* (London, 1967), 305–06.

³⁶ From the *Clarion*, July 24, 1908, quoted in Fenner Brockway, *Socialism Over Sixty Years, The Life of Jowett of Bradford* (London, 1946), 73.

³⁷ Brockway, *Jowett*, 104.

would leave unresolved, however, the further question of whether that unrest was in any way connected with concurrent agitations over Ireland and women's rights. An answer may lie in the working man's alienation from the law and from Parliament. A glance at the history of the period is enough to confirm that suffragettes and Ulsterites shared the same disaffection. Both groups considered themselves pushed by circumstances to a desperation that turned those stately institutions into irrelevant stumbling blocks. Indeed almost all that the Victorians had claimed to cherish and had been content to take for granted was in at least some measure called into question by Edwardian and Georgian facts of life. The Boers and the German navy threatened Britain's defense establishment and, worse, her very security. International competition menaced free trade. Property confronted the land tax and employers' liability. Family felt the sting of a ruthless cost of living as well as the blows of feminist warriors. Bloomsbury considered self-discipline and duty and proclaimed them repression and self-deceit. Wilde transformed the serious business of life into a quest for muffins and cucumber sandwiches. All this sent shock after shock right through society. The more stable the institution or value, the more traumatic the challenge when it came.

Working men and women found themselves, like everyone else, struggling to come to terms with a social system and a set of values that they knew were changing fast. Not only, for example, did their class-consciousness increase ("we" against "they"); the structure of the working class itself was undergoing considerable readjustment. Victorian England tended to divide the working class into two categories—artisans and laborers—a division the working class itself accepted along with everyone else, since it appeared to conform to the facts. The artisans were the men with a skill or a trade. They earned a wage that allowed them to live decently and, if there were not too many young children, in some degree of comfort. The laborers had only their strength to sell. They were the hewers and drawers, and in many industrial towns over forty per cent of their number lived in stony poverty. The line between the two castes was wide, wider than that which separated the skilled artisans from the clerks and small shopkeepers of the lower middle class. John Burns contrasted the "old" unionists (the skilled) with the "new" (the unskilled) as they appeared at the Trades Union Congress of 1890.

Physically the "old" unionists were much bigger men than the "new," and that, no doubt, is due to the greater intensity of toil during the last twenty or thirty years . . . the "old" delegates differed from the "new" not only physically but in dress. A great number of them looked like respectable city gentlemen; wore very good coats, large watch-chains and high hats—and in many cases were of such splendid build and proportions that they presented an aldermanic, not to say a magisterial dignity.

Amongst the "new" delegates not a single one wore a tall hat. They looked workmen. They were workmen. They were not such sticklers for formality or Court procedures, but were guided more by common sense.³⁸

At past congresses the "old" men had ruled absolute. Indeed until very recent years there had been no "new" men to raise a fuss. At the very meeting Burns describes, however, the "new" men overthrew the "old." They forced through a program of socialist demands and effected the resignation of that champion of the "old," Henry Broadhurst.

Remarks of this nature argue in favor of the theory of a labor aristocracy. One equates the "old" with the "aristocrats," better paid, with a history of steady employment in a skilled industry, and content with political Lib-Labbery. This profile conforms generally to that delineated by Eric Hobsbawm in his influential article on the subject. Using as criteria "prospects of social security," "conditions of work," "relations with social strata above and below him," "general conditions of living," "prospects of future advancement," and—"incomparably the most important"—"the level and regularity of the workers' earnings,"³⁹ he has argued that one can distinguish an aristocracy that until the beginning of the twentieth century lived closer to a lower-middle than to a working-class life. Pelling challenges the argument in *Popular Politics*.⁴⁰ Hobsbawm, as a Marxist, finds the theory of a labor aristocracy a comforting one. It stands, says Pelling, as a convenient explanation of "the signs of comparative affluence in the working class, and also the presence of non-revolutionary sentiments among the workers." Pelling faults Hobsbawm for defining the aristocrats almost exclusively in terms of their wages and therefore in terms of the nature of their employment.

The wages of the individual worker do not readily provide us with an index of his relative affluence, which must depend upon the size of his family, the earnings, if any, of his wife and children, the ability of his wife as a housekeeper, and his and her financial self-discipline, foresight, intelligence, and temper.⁴¹

He contends as well that to use the concept of an aristocracy to explain working-class political quiescence flies in the face of facts which suggest that, on the contrary, it was in most cases the aristocrats—engineers like Burns, Mann, and J. L. Mahon—who were the most militant.

How can one sort all this out, in the face of accounts such as Burns's? Certainly working men themselves recognized distinctions within their own ranks, based upon all the criteria Hobsbawm lists and tempered by the circumstances Pelling cites. Further, as the two historians acknowledge, the distinctions were beginning to blur with increasing rapidity

³⁸ Quoted in Raymond Postgate, *The Builders' History* (London, 1923), 343.

³⁹ Eric Hobsbawm, "The Labour Aristocracy in Nineteenth-Century Britain," in his *Labouring Men* (London, 1964), 273.

⁴⁰ Pelling, "The Concept of the Labour Aristocracy," in *Popular Politics*, 38.

⁴¹ *Ibid.*, 52.

at the beginning of the twentieth century. Hobsbawm, the Marxist, suggests that imperialist capitalism created an "alternative hierarchy" of civil servants, clerks, and teachers that alienated the labor aristocracy from the lower middle class, of which it had been able to feel itself a part.⁴² Hobsbawm seems to imply—although he nowhere asserts—that the result was a growing willingness on the part of labor aristocrats to identify with the interests of their less affluent brothers.⁴³

Both Hobsbawm and Pelling discuss a second and more readily substantiated reason why the working class was growing from two classes into one: technology was making it difficult to distinguish with any assurance the skilled from the unskilled. As Pelling puts it, "The process of change gradually invaded separate crafts one after the other, reducing them all, or nearly all, to a situation in which the skill of the worker was something that could readily be 'picked up'."⁴⁴ Hobsbawm maintains that "the competition of machinery and the threat of down-grading" compelled some of the most "aristocratic" unions to affiliate with the Labour Representation Committee.⁴⁵ Burns spoke of this threat, from the point of view of the "new" and the "old," in his account of the Liverpool Congress.

There has been a lot of cant talked about the "new" and the "old" trades unionism. The difference between them, if any, is entirely due to the fact that the "old" see that labour-saving machinery is reducing the previously skilled to the level of unskilled labour, and they must, in their own interests, be less exclusive than hitherto. The "new" believe that distinctions of labour must disappear and that class prejudices that have disintegrated the Labour movement in the past must be abolished.⁴⁶

The continuing introduction of new technology brought with it a breakdown of the apprenticeship system and of the implication that a man once taught his craft need never relearn it. It meant that the "unskilled," when put to work at a machine, became to that extent "skilled" and therefore indispensable. It brought the use of new materials—concrete, for example—with which the older skilled workmen were wholly unfamiliar. It dictated the substitution of cooperative for individual effort—vividly illustrated in Charles Booth's description of a locomotive

⁴² The number of clerks in London rose from 90,000 in 1881 to 172,000 in 1911. Paul Thompson, *Socialists, Liberals, and Labour: The Struggle for London, 1885-1914* (London, 1967), 14. Between 1880 and 1914 the class of occupied people receiving an "intermediate income" more than doubled, whereas the increase in the occupied population as a whole was 40 per cent. Clegg, Fox, and Thompson, *History of Trade Unions*, 1: 473.

⁴³ G. D. H. Cole, however, reads the evidence a different way in his *Studies in Class Structure* (London, 1955), 39. He believes the increased number of clerks blurred the distinctions among classes. "The upshot was a much greater differentiation within the classes of employed persons, and a blurring of the lines of division between wage- and salary-earners, and therewith between 'workers' and the 'lower middle class.'"

⁴⁴ Pelling, "Concept of the Labour Aristocracy," 45-46.

⁴⁵ Hobsbawm, "Labour Aristocracy," 289.

⁴⁶ Postgate, *Builders' History*, 343-44.

factory.⁴⁷ And it plagued artisans accustomed to working at their own pace with the hated time and motion studies of scientific management.⁴⁸

Standardization, subdivision, and mechanization disrupted working-class patterns and habits by drawing the skilled and unskilled together. The experience was bound to upset those caught up in it. And its effects manifested themselves in the strikes the workers felt increasingly compelled to call. More often than not, status was at stake, along with wages. As the unskilled worker acquired a position of some importance and security within his class, he became the more anxious to insure that black legs did not wrest it from him. Dangerfield estimates that during 1910 only 20 per cent of the strikes concerned themselves with wages; the rest arose over conditions of labor and refusal to work with non-unionists.⁴⁹ The dockers' major claim in 1912 was the compulsory union ticket. The rank-and-file railwaymen considered recognition a more vital goal than wage conciliation; their leaders joined the Triple Alliance from a conviction that management was systematically harassing their membership.

Status meant more than keeping the black legs at bay. There were as well the questions of the new unionists' relationship with their more "aristocratic" brothers and, among those brothers, of the need to adjust to the breakdown of specialization by craft. Unions had to redefine work patterns that had been drawn under far different conditions than those existing by 1900. Booth, writing in the nineties of the need for union regulation between workman and workman, cited

the cases of the artisan or mechanic and the assistant labourer, when the latter is forbidden to use the skilled man's tools; in the definition, or attempted definition of the spheres of work of such trades as shipwrights and ships' carpenters; of mason and bricklayers where they come together, or of plumbers and fitters.⁵⁰

Postgate, in his *Builders' History*, takes the plumbers as an instance in point.

They were unable to grapple with the difficulties involved in the new processes; their only remedy was to claim that whatever had before been done by plumbers should still be done by them, though iron or china had taken the place of lead. In 1903 they recorded, without dissatisfaction, that they had longstanding and

⁴⁷ Charles Booth, *Labour and Life of the People of London* (3d ed.; London, 1891), 9: 227.

⁴⁸ See Phelps Brown, *British Industrial Relations*, 92-98; Eric Hobsbawm, "Customs, Wages, and Work-load in Nineteenth Century Industry," in *Labouring Men*, 344-70.

⁴⁹ Dangerfield, *Strange Death*, 247. Phelps Brown argues that there was a decline in the usefulness of boards of conciliation and arbitration, since they were designed primarily to resolve questions of wages, not status. As status began to divide labor and management after 1900, Phelps Brown maintains that these boards, which depended on some common ground between the two sides in order to succeed, ceased to function effectively. *British Industrial Relations*, 143-44, 340. Knowles, however, counters with the fact that "between 1910 and 1913 the yearly average number of strikes was over twice as high as it had been between 1904 and 1909; but the number of cases settled by conciliatory methods was almost three times as high." Knowles, *Strikes*, 66 n.1.

⁵⁰ Booth, *London*, 9: 252-53.

bitter consequent quarrels with no less than five other crafts—whitesmiths, hot-water fitters, zinc workers and glaziers.⁵¹

Amidst these confusions over status and jurisdictions, as Phelps Brown points out, working men found it more and more difficult to determine the issues at stake or to take them to their union leadership for solution.⁵² Hence the more than usual number of wildcat strikes during the period. The rank and file grew impatient with leadership too often insensitive to particular local difficulties and distinctions. They resented trade-union bureaucracies (here again the matter of status was an issue) that sent out agents from comfortable London headquarters to investigate conflicts whose bitterness they could not share and to propose conciliatory solutions the workers would not accept. Workers refused to sacrifice local momentum while awaiting a decision from London; South Wales miners disparaged centralized conciliation as “class collaboration.”⁵³ Too often, on the other hand, rigid regulations combined with a lack of ready talent to prevent the growth of any sort of flexible negotiating system at the local level. The resulting conflicts were regularly expressed at annual meetings of the Trades Union Congress. In 1911 the cautious strike policy of the Parliamentary Committee, which blamed violence on the police and army but spoke out strongly in favor of discipline and centralization, met with defeat at the hands of the delegates. They passed instead a resolution declaring that “no effort shall be spared by the forces of organized labour to arouse and maintain the discontent of underpaid workers with their conditions, and to quicken and assist their determination to use all possible means to win for themselves a living wage.”⁵⁴ They attacked as well a proposal supported by the Labour leadership, which called for a thirty-day notice prior to the commencement of a strike.

Uncertain as to what its place should be within the working-class hierarchy, the rapidly expanding rank and file picked a fight with its leaders over what their place had been. At stake was not so much union policy and procedure as a redefinition on the part of the working class of its own social structure. The problem was complicated by the continued prevalence of marked and galling contrasts between working-class families and neighborhoods, which neither technology nor fulminations of the Trades Union Congress had abolished. Some workers lived comfortably; one-third

⁵¹ Postgate, *Builders' History*, 378–79.

⁵² Phelps Brown, *British Industrial Relations*, 231.

⁵³ Page Arnot, *The Miners*, 322. See also his account of the difficulties encountered by the union leadership in settling the Cambrian strike of 1911. *The Miners*, 2: 71.

⁵⁴ From the *Annual Report, 1911*, quoted in Roberts, *Trades Union Congress*, 241.

⁵⁵ Of those members of the working class in the 1890s whose weekly income was less than thirty shillings, D. J. Oddy has this to say: “The individuals in these groups ate only six pounds of bread per week, and the whole diet provided less than 2000 kilo-calories a day. On the whole, what they ate was very similar to the diet of the worst group [needlewomen in London] examined by Dr. Edward Smith in 1863, except that sugar consumption was double that of the 1860's.” D. J. Oddy, “Working Class Diets,” *Economic History Review*, 23 (1970): 320.

lived below the poverty line.⁵⁵ A growing number sat on city councils; a majority still did not have the vote.⁵⁶ A handful won places in secondary schools; twelve per cent of the Middlesbrough working class could neither read nor write.⁵⁷ The distinctions and the jealousies engendered by "rising expectations" did nothing to simplify the job of social redefinition.

That task was but one of several workers faced as they tried to make twentieth-century sense of nineteenth-century institutions. Another was the acceptance of the changing circumstances of welfare and charity and of changing attitudes toward social service. The years 1890 to 1914 witnessed the climax of that struggle Asa Briggs has described, between the theories of the social-service state and the welfare state. Victorian England had been a social-service state, in which the upper and middle classes, through their own or the government's agency, extended helping hands to the deserving poor. Modern England is a welfare state, in which the government offers "all citizens without distinction of status or class . . . the best standards available in relation to a certain agreed range of social services."⁵⁸ Edwardian England straddled the two eras: the Charity Organization Society, the most sophisticated rationalization of the old; the Webbs' Minority Report, a daring blueprint for the new. And the working class was by no means ready to exchange the former for the latter.

Pelling has given us convincing evidence on that point. A chapter in *Popular Politics*⁵⁹ catalogs working-class opposition to the extension of state power, "which is generally regarded as having laid the foundations of the welfare state."⁶⁰ He lists the Poor Law of 1834, the Housing Acts of the 1870s, and the Education Acts of 1870 and 1880 as examples of interventionist legislation that met with working-class hostility, or indifference, or both. Such opposition, Pelling points out, went hand in hand with working-class mistrust of the law. Certain trades welcomed government-imposed safety regulations and some, but not all, unions supported bills delimiting the factory working day.⁶¹ Neither political party won working-class support by promulgating elaborate programs of social reform.

When the Liberals did introduce pensions, insurance, and labor ex-

⁵⁶ Only 60 per cent of the males of voting age had the vote after 1885. N. Blewett, "The Franchise in the United Kingdom, 1885-1918," *Past and Present*, 32 (1965): 31.

⁵⁷ Lady Bell, *At The Works* (London, 1969; 1st ed., 1907), 162.

⁵⁸ Asa Briggs, "The Welfare State in Historical Perspective," *Archives Européennes de Sociologie*, 2 (1961): 228.

⁵⁹ Pelling, "The Working Class and the Origins of the Welfare State," in *Popular Politics*, 1-18.

⁶⁰ *Ibid.*, 2.

⁶¹ Page Arnot recounts the astonishment of Continental delegates to the Miners' International in Brussels in 1893, when two Englishmen opposed a motion to affirm the principle of the legal eight-hour day. "Both were working miners from Northumberland; and both were convinced that Parliament could not 'righteously and justly interfere with the hours of adult labour.' This reduced Emile Basley of France to 'a state of wonderment.' The amendment meant, in four words, 'objection to state intervention': from this he had presumed the representatives of Northumberland were anarchists. . . . 'What,' he asked, 'was the use of trade unionists sending representatives to parliament if, when they got there, these "anarchist" representatives were not to legislate at all?' " Page Arnot, *The Miners*, 1: 172.

changes, their progressivism, as we have seen, bewildered more often than it delighted the working man. The Liberal innovation most popular with the working class—old-age pensions—incurd the bitter opposition of the Friendly Societies, which saw them, as they saw national insurance, as an unwarranted and essentially unnecessary threat to their existence.⁶² All other welfare schemes encountered at least some measure of opposition when proposed, and confusion and disappointment when enacted.⁶³ Intervention too often appeared to the working class as another name for interference.⁶⁴ The Poor Law had taught them that. Lady Bell, in her description of the lives of Middlesbrough iron workers, reports local disgust at a government attempt to regulate and hence improve the practice of midwifery through licensing. Since many of the midwives either forgot or refused to bother to pay their guinea fee, the practical result of this well-intentioned reform was to reduce the number of the town's practitioners.⁶⁵ Lady Bell concluded that social reformers lose more than they gain when they prod the worker to accept improvements he has not first been tutored to understand.

One learns after long experience that what he wants is that the next step that is of any concern to him, and that he chooses to take in his own way, should be facilitated for him; but one has to be very careful not to go a little further and make superfluous suggestions which he then, much to the discomfort of the suggester, absolutely negatives and dismisses.⁶⁶

The worker had to be weaned from the belief that he would be the weaker for having accepted relief and made to see that the country might be the stronger if he would only accept his government's support. Throughout the nineteenth century the English middle class did its best to see that he learned that first lesson; now, in the twentieth, its reform-minded heirs pressed him to understand the second. That their task of re-education was arduous and frustrating speaks eloquently of the thoroughness with which the Victorian bourgeoisie catechized its working-class pupils.

Two further examples will suggest the breadth of this problem of readjustment and adaptation that the working class, along with everyone else, was forced to confront. Richard Hoggart has remarked that "the more we try to reach the core of working-class attitudes, the more surely does it appear that that core is a sense of the personal, the concrete, the local; it is embodied in the idea of, first, the family, and, second, the neighborhood."⁶⁷

⁶² Bentley B. Gilbert, *The Evolution of National Insurance in Great Britain* (London, 1966), 160.

⁶³ See, for example, Page Arnot, *The Miners*, 2: 108, 129–21, for the disappointed reaction of the miners to the Minimum Wage Act of 1912.

⁶⁴ Though the unions did learn that by forcing government intervention as a third party in an industry-wide strike, they could sometimes achieve victory over their employers. The miners proved the point in 1912, when they pressured the government into passing a minimum wage bill.

⁶⁵ Bell, *At the Works*, 265.

⁶⁶ *Ibid.*, 98.

⁶⁷ Richard Hoggart, *The Uses of Literacy* (Boston, 1961), 32.

One discovers that core at the center of Edwardian working-class life, constantly threatened, however, by a modern sense of urban alienation. Many working men and women were born and grew up far from their subsequent homes—either in the country, in another city, or in another quarter of the same city in which they later lived. Hope of better wages, the vicissitudes of urban renewal, a growing family, the lack of work near to hand, the availability of a workers' train—these and other factors, singly or in combination, were forcing more and more members of the working class to move about. The more they moved, the more difficult it became to combat the rootlessness and loneliness of a series of flats or tiny houses, generally alike, but not for that reason any the more familiar or comforting. They fought back with ritual, creating rhythms from the events of the present and epics of everyday occurrences of the past.

There is a rhythm, but it is the rhythm of a brick world, to which those of the seasons or of the great religious festivals are only incidental. At each weekend, perhaps, there is Friday night's shopping with Mother down a shopping street that is all bustle and warmth and gregarious spending, and the trains rattle and flash past constantly. There is the whole weekend ahead, with the pictures on Saturday, or a chapel concert with a hot supper in the Sunday school room; bacon and eggs for Sunday breakfast, the big Sunday tea. Then, throughout the year, Pancake Tuesday, Voting Day, which is always a holiday, Hotcross Buns on Good Friday, the Autumn "Feast," Mischief Night, and all the weeks of cadging and collecting for Bonfire Night. It is a truly urban fire, with very little wood that has known a tree for the past few years. . . . As the fireworks run out, you bake potatoes round the fire's edges.⁶⁸

Remove the reference to pictures on Saturday, and the rhythm is much like that Edwardian working-class families maintained as they faced down the facts of urban anonymity and psychological nakedness.

The rhythm implied an allotted time for pleasure. Edwardians practiced enjoyment as Victorians had preached self-denial. Here was another readjustment, and an especially difficult one for the working class to make. Once rich men and women ceased to be afraid of ostentation, they had only to begin spending the capital their Victorian forebears had felt constrained to leave untouched. A working man, on the other hand, soon found to his sorrow that to desert thrift for pleasure, even to the extent of a glass or two of beer, was to put his entire family in jeopardy. Wages of twenty-five shillings per week, each halfpenny budgeted for the necessities of life, allowed for no such indulgences.⁶⁹ Economic circumstances

⁶⁸ *Ibid.*, 59.

⁶⁹ Rowntree's famous passage describing the amenities one would have to do without if one wished to live above the poverty line makes the point. "A family living upon the scale allowed for in this estimate must never spend a penny on railway fare or omnibus. They must never go into the country unless they walk. They must never purchase a halfpenny newspaper or spend a penny to buy a ticket for a popular concert. They must write no letters to absent children, for they cannot afford to pay the postage. They must never contribute anything to their church or chapel, or give any help to a neighbour which costs them money. They cannot

forced him to deny himself the pleasures others were beginning to take for granted and enjoy. Lady Bell deplored the fact that no one taught the working class "the great, the much-needed lesson for both old and young, the need for self-control both in word and deed."⁷⁰ The middle classes, schoolmasters to the poor, were ceasing to believe the lesson. Left to themselves and encouraged by propaganda such as Robert Blatchford's *Merrie England*,⁷¹ workers as often as not failed to learn it, until poverty descended to teach it in perhaps the only way it could still be taught.

EVERY AGE IS an age of transition, but this was transition with a vengeance. Social institutions will hold people together only as long as the people believe in the institutions' worth or in their existence. Events brought Victorian institutions into question; once questioned, they ceased to bind Englishmen as they had before. Centrifugal force—the pull away from a common acceptance of essentially middle-class Victorian assumptions—spun the English off into the separate confusions that mark the period. The Ulsterites, the suffragettes, the workers, to a lesser degree the rest of their countrymen, were looking for ways to cope with the reality of change. In this, their "revolts" had common roots, and their uncertainties fed each other. As Phelps Brown remarks: "Events which stir the emotions in the same way boost one another's signals when they are in circuit with one another, and their aggregate effect is greater than the sum of the effects they would take separately."⁷²

The working men's "sense of an impending clash" sprang, then, from their sense of economic grievance and of class-consciousness. It was almost certainly heightened by their understandable confusion at the rapid disappearance of Victorian institutions they had taken for granted. They were upset and not at all sure what to do. So they did many apparently contradictory things at once: they struck for wages and struck for status; they sent the TUC their money and bucked its leadership; they opposed na-

save, nor can they join sick club or Trade Union, because they cannot pay the necessary subscriptions. The children must have no pocket money for dolls, marbles, or sweets. The father must smoke no tobacco, and must drink no beer. The mother must never buy any pretty clothes for herself or for her children, the character of the family wardrobe as of the family diet governed by the regulation, 'Nothing must be bought but that which is absolutely necessary for the maintenance of physical health, and what is bought must be of the plainest and most economical description.' Should a child fall ill, it must be attended by the parish doctor; should it die, it must be buried by the parish. Finally, the wage-earner must never be absent from his work for a single day." B. Seebohm Rowntree, *Poverty, A Study of Town Life* (London, 1910), 133-34.

⁷⁰ Bell, *At the Works*, 220.

⁷¹ Blatchford included pleasure among the "mental needs of life." "As for pleasures, their name is legion. There are such pleasures as walking, rowing, swimming, football, cricket. There are the arts, and the drama. There are the beauties of nature. There are travel and adventure. Mere words cannot convey an idea of the intensity of these pleasures." *Merrie England* (New York, 1966; 1st ed., 1894), 20.

⁷² Phelps Brown, *British Industrial Relations*, 332.

tional insurance while they collected their pensions; they voted Labour and agreed that Labour accomplished almost nothing.

Dangerfield to the contrary, there was little joy in them as they surveyed their strange-seeming world. He is mistaken when he describes them as "wanting to live, to take chances, to throw caution to the winds."⁷³ One does catch this sense in the memoirs of some of the intellectual rebels of the time. Leonard Woolf describes the excitement of taking part "in the spring-time of a conscious revolt against the social, political, religious, moral, intellectual, and artistic institutions, beliefs, and standards of our fathers and grandfathers." "It seemed," he writes, "as though human beings might really be on the brink of becoming civilized. It was partly the feeling of relief and release as we broke out of the fog of Victorianism."⁷⁴ Few members of the working class shared that *élan*. Trapped between a consciousness of "rising expectations" and an atavistic fatalism that denied the possibility of change, they could not subscribe wholeheartedly to a belief in progress and a better future. The past, though a matter of dreary, impoverished sameness, was nevertheless a matter of certainty.

Was it not uncertainty, even fear perhaps, that as much as anything drove the English worker to act as he did in those years before 1914, to spend as much time as he did on the defensive?⁷⁵ Until historians have studied authentic working-class sources more thoroughly than they have, the suggestion can be no more than that. A final clue may lie, however, in the words with which Barrington Moore concludes his *Social Origins of Dictatorship and Democracy*. "The wellsprings of human freedom lie not only where Marx saw them, in the aspirations of classes about to take power, but perhaps even more in the dying wail of a class over whom the wave of progress is about to roll."⁷⁶

⁷³ Dangerfield, *Strange Death*, 235.

⁷⁴ Leonard Woolf, *Beginning Again* (London, 1964), 34, 36.

⁷⁵ Stearns argues that French workers, especially during the period 1905–10, struck most commonly in defense of what they had managed to gain in the past rather than in anticipation of what they might achieve in the future. "A large number of strikes that apparently sought higher wage rates in reality were defending accustomed levels. . . . French workers had obviously improved their standards of living in the preceding decades, but they still had not collectively displayed a commitment to steady progress in the future." *Revolutionary Syndicalism*, 46. The same might well have been true of English workers.

⁷⁶ Barrington Moore, *The Social Origins of Dictatorship and Democracy* (Boston, 1967), 505.

Stalin and the Prospects of a Separate Peace in World War II

VOJTECH MASTNY

THE FEAR THAT Moscow and Berlin might again come to terms preoccupied American and British statesmen long after Hitler had forced the unwilling Stalin to join the Allied coalition. In the opinion of George F. Kennan, a touchstone for appraising the wartime policies of the Americans and the British "will be found . . . in the soundness and accuracy of their fears with relation to the possibility of a separate German-Soviet peace."¹ The possibility never materialized; but does that mean a Russian-German rapprochement was merely an empty threat? This is indeed an important question, for the answer may influence the appraisal of both the Soviet war aims and the Western responses to them—two central themes in the current debate about the origins of the cold war.

If Stalin contemplated a separate peace at any particular moment during the war, his objectives must have been more flexible than they appear in retrospect. Any compromise with Germany would inevitably have given him considerably less territory and influence than he achieved in 1945. But was there ever a favorable situation for a compromise? And even if there was, would the Russians have discussed peace with Hitler, or would they have required his replacement by a non-Nazi government first? In either case, how much did the prospect of an armistice on the eastern front influence the policies of the Western allies? Was not Stalin himself creating a false alarm in order to exact concessions from his coalition partners?

These intriguing questions have so far received surprisingly little scholarly attention. The late Herbert Feis, for example, all but ignored them in his history of the Great Alliance.² William H. McNeill has confined his opinion to a few cautious remarks.³ In the 1950s John A. Lukacs was the only one to come to grips with the problem as far as his limited evidence then

¹ George F. Kennan, *Russia and the West under Lenin and Stalin* (Boston, 1961), 362–63.

² Herbert Feis, *Churchill, Roosevelt, Stalin: The War They Waged and the Peace They Sought* (Princeton, 1967), 143.

³ William H. McNeill, *America, Britain, and Russia: Their Co-Operation and Conflict* (London, 1953), 275, 324.

allowed; he concluded that Moscow had considered the alternative of a separate peace seriously.⁴ A few other authors have subsequently written about it, but none has done so within the broad context the subject deserves.⁵ The shortage of reliable evidence has been the principal obstacle for historians. All their inquiries have so far depended almost exclusively upon the testimony of Peter Kleist, the less than reputable former aide of the Nazi foreign minister, Joachim von Ribbentrop. In his postwar memoirs Kleist maintained that during the war he had received several Soviet peace feelers in Stockholm.⁶ German authors have been somewhat more inclined to take his word than have their foreign colleagues but have done little to substantiate their judgment.⁷ It is to be regretted that the conclusive evidence from the Moscow archives may not be available for some time to come. In the meantime, however, more than has previously been known can be deduced from additional sources accessible in the West. Unpublished records of the United States Department of State and Department of the Army—particularly the military intelligence papers—captured German documents, and little-known publications from Eastern Europe are among the sources used in this essay for the first time.⁸

From the Soviet point of view a peace with Berlin would have necessarily meant two very different things before and after Stalingrad. During the period of German ascendancy it could only have entailed strategic surrender—"the orderly capitulation of the remaining forces" in order to prevent the worse consequences of a defeat.⁹ After Stalingrad had reversed military fortunes a genuine compromise was theoretically possible. Accordingly peace prospects during the periods preceding and following that crucial turning point of the war ought to be considered separately.

No sooner did Hitler and Stalin begin to fight than they were suspected of seeking a settlement. The United States assistant secretary of state, Adolf A. Berle, Jr., for example, considered their rapprochement possible as early as July 1941; the British Embassy in Moscow expected a Soviet bid for peace

⁴ John A. Lukacs, *The Great Powers and Eastern Europe* (New York, 1953), 502-24.

⁵ See, for example, Karl-Heinz Minuth, "Sowjetisch-deutsche Friedenskontakte," *Geschichte in Wissenschaft und Unterricht*, 16 (1965): 38-45.

⁶ The English translation, entitled *The European Tragedy* (London, 1965), is an expanded version of the original edition of Peter Kleist, *Zwischen Hitler und Stalin, 1939-1945* (Bonn, 1950).

⁷ For example, Lothar Gruchmann, *Der Zweite Weltkrieg: Kriegführung und Politik* (Munich, 1967), 242-43; and Boris Meissner, *Russland, die Westmächte und Deutschland* (Hamburg, 1954), 13-17.

⁸ The following archival sources will be cited throughout this essay: Records of the Office of Strategic Services, record group 226, National Archives, Washington (hereafter OSS); Records of the War Department, General and Special Staffs—Military Intelligence (G-2), record group 165, Washington National Record Center (hereafter G-2); General Records of the Department of State, record group 59, National Archives (hereafter DS); Records of the German foreign office (Auswärtiges Amt), microcopy T-120, National Archives (hereafter AA); Records of the British Foreign Office, General Correspondence, FO 371, Public Record Office, London (hereafter FO); and Records of the British War Cabinet, Memoranda, CAB 66, Public Record Office, (hereafter CAB).

⁹ Paul Kecskemeti, *Strategic Surrender: The Politics of Victory and Defeat* (New York, 1964), v.

two months later.¹⁰ In early 1942 London was especially apprehensive about the chances of a sudden armistice.¹¹ Lord Halifax, the ambassador to Washington, repeatedly expressed his concern in private conversations.¹² For reasons inherent in the Soviet system, a peace initiative from Moscow was not out of the question. The Bolsheviks had proved before that the ideological antagonism between nazism and communism did not necessarily rule out collaboration. And the Russian government was under no obligation to its constituents to maintain the alliance with the West. The autocrat, who had all decisions about war and peace in his hands, could enforce his will easily, regardless of public opinion, if he chose to do so. Extensive freedom from domestic restraints was but one of the many affinities between Stalin and Hitler—the two dictators who hated but respected each other. Of the two, it was Stalin rather than Hitler who tended to underestimate the incompatibility of their respective interests. Stalin personally had a greater share than Hitler in bringing to conclusion the pact of August 1939, had benefited from it more than his Nazi counterpart, and had therefore tried to preserve it until the very last moment.

As a matter of fact, Soviet spokesmen expressed nostalgia for a *modus vivendi* with Berlin even after the Nazis had already violated it. On June 24, 1941, for example, the emigré leadership of the German Communist party in Moscow issued a statement in favor of an "indestructible alliance" between the German and Russian peoples.¹³ Two days later Walter Ulbricht, that particularly faithful interpreter of Stalin's thoughts, drafted an appeal to Hitler's soldiers in which Ulbricht drew a sharp distinction between their unjustified attack on the "Fatherland of Socialism" and their struggle against the Western "plutocracies."¹⁴ Although such explicit statements soon ceased, they nevertheless suggest that Stalin would have regarded a renewed understanding with Germany as both feasible and desirable. From his point of view an arrangement with the congenial Nazi dictator could still have been easier than dealing with the Western statesmen whose thinking was so alien to his own and who were particularly reluctant to grant him title to the territories he had seized in 1939 and 1940.

In early 1942 the British government was actually quite tempted to endorse Stalin's territorial claims in Eastern Europe in order to avert his

¹⁰ Adolf A. Berle, Jr. to J. Edgar Hoover, July 10, 1941, *Foreign Relations of the United States* (hereafter *FRUS*), 1941, 1: General, *The Soviet Union* (Washington, 1958), 789-90; Sumner Welles, "Two Roosevelt Decisions: One Debit, One Credit," *Foreign Affairs*, 29 (1950-51): 189.

¹¹ Llewellyn Woodward, *British Foreign Policy in the Second World War*, 2 (London, 1971): 239, 244.

¹² S[umner] W[elles], memorandum on conversation with Lord Halifax, Mar. 30, 1942, *FRUS*, 1942, 3: *Europe* (Washington, 1961), 537. See also Jan Ciechanowski, *Defeat in Victory* (New York, 1947), 109.

¹³ Statement printed in the journal *Kommunisticheskii International* (1941), no. 6-7, quoted in report no. 44338 C, OSS.

¹⁴ Walter Ulbricht, "Entwurf eines Aufrufes an die deutschen Soldaten," June 26, 1941, in his *Zur Geschichte der deutschen Arbeiterbewegung: Aus Reden und Aufsätzen*, 2: (1933-1946), supp. 2 (Berlin, 1968): 221.

possible defection. Churchill explained the reasons to Roosevelt in unequivocal terms: "The increasing gravity of the war has led me to feel that the principles of the Atlantic Charter ought not to be construed so as to deny Russia the frontiers she occupied when Germany attacked her."¹⁵ But Washington objected on both moral and practical grounds and insisted that any reference to the controversial frontiers be deleted from the final text of the British-Soviet treaty, which was signed on May 26, 1942.¹⁶ The Americans were loath to bestow an air of legitimacy upon the annexations that Stalin had carried out in a particularly scandalous fashion during his collusion with Hitler. In addition they were trying to avoid the Wilsonian predicament by abstaining from any commitments that might prove embarrassing after the war.

The policy of refusing to abet Stalin's misdeeds had unquestionable virtues, but the promotion of his faith in the alliance was not one of them. On December 6, 1941, the Soviet leader told the British foreign secretary, Anthony Eden, "If our war aims were different, then there would be no alliance."¹⁷ Rightly or wrongly Stalin considered the "second front" the supreme proof of cooperation,¹⁸ and in his mind its absence could even have cast an altogether sinister light upon the extensive material aid he was getting. Were the Western capitalists not sending him just enough to promote a military stalemate in order to benefit later from the mutual exhaustion of both belligerents? Such a putative scheme must have seemed especially plausible to Stalin, since he himself had thought along these lines in 1939-41, as the shifting emphases of the Comintern's statements at that time indicate.¹⁹ And it should be remembered that the Russians, in conformity with Marxist doctrine as they understood it, vastly exaggerated the influence that British aristocrats and American financiers who were hostile to the Soviet Union were able to exercise upon the policies of their respective governments.

In this context it matters little that political considerations did not in fact cause the repeated postponements of the landing on the Continent;²⁰ in all fairness some Western actions could have hardly failed to create an impression of duplicity. The Russians were perhaps understandably disturbed about the inability of the British to give a convincing explanation of the awkward peace mission of Rudolf Hess—admittedly a difficult thing to do—and about the unwillingness of the London government to bring the

¹⁵ Churchill to Roosevelt, Mar. 7, 1942, in Churchill, *The Hinge of Fate* (London, 1951), 293.

¹⁶ See the treaty in Louise W. Holborn, ed., *War and Peace Aims of the United Nations*, 1 (Boston, 1943): 235-37.

¹⁷ Stalin, quoted in Woodward, *British Foreign Policy*, 2: 223.

¹⁸ The official Soviet view is summarized in V. M. Kulish, *Vtoroi front* (The Second Front) (Moscow, 1960), 16-20.

¹⁹ The Comintern's statements are analyzed in Kurt Krupinski, *Die Komintern seit Kriegsausbruch* (Berlin, 1941).

²⁰ Evidence that it was not political considerations that postponed the Allied landings appears in Maurice Matloff and Edwin S. Snell, *Strategic Planning for Coalition Warfare, 1941-1942* (Washington, 1953), 217-32, 322-27.

self-appointed intermediary to trial.²¹ Even more disconcerting was Roosevelt's unfulfilled promise to launch the invasion of Europe before 1943, a promise he had let Molotov believe was definite.²² Then, too, no matter how genuine the president's desire to promote mutual trust, his volubility may have had the very opposite effect on Soviet minds ever disposed to suspect the worst.²³

Thus Stalin had reasons, albeit the wrong ones, to question the motives of his allies in 1942. Yet these alone did not necessarily prompt him to desert them. Very soon after June 1941 the Nazis proved by their behavior that so long as they retained the upper hand the only peace terms the Soviet Union could expect would be complete submission. Russia's sole alternative was to fight on, and any signs of faltering resolve would have, if anything, given further encouragement to the enemy; or even worse, they might make the dreaded Western reversal of alliances a self-fulfilling prophecy. Stalin could have been exasperated with the coalition, but he had to maintain it. On balance, therefore, any Russian efforts to come to terms with Germany before Stalingrad may be dismissed as mere products of anxious imagination.

DURING THE WINTER of 1942-43 the situation changed radically. Having compelled the Nazis to retreat from the Caucasus, the Red Army encircled German forces at Stalingrad on November 23, 1942, and annihilated them by February 1, 1943. The power of the two belligerents now seemed to be almost balanced—a condition that E. A. Boltin, a leading Soviet military historian, believes lasted until mid-1943.²⁴ But although Moscow's military prospects brightened, a complete defeat of the enemy was still in the distant future, especially as long as the second front remained uncertain. Past combat experience gave every reason to believe that the path of victory would be arduous, and the Russians had already suffered appalling losses. Yet the military situation after Stalingrad offered the Soviet Union, for the first time since the beginning of the war, both the opportunity and the inducement to trade military assets for a political compromise. Soviet leaders, well versed in the Marxist notions about the interaction of war and politics, could have hardly missed that message. And indeed, an authoritative article published in January 1943 in the party journal *Bolshevik* indicated that they did not. Its author was none other than Colonel E. Razin, whom Stalin had chosen on other occasions as mouthpiece for his own views about military questions. The colonel affirmed that "separation of politics and strategy, and the neglect of the requirements of politics for 'purely stra-

²¹ Woodward, *British Foreign Policy*, 2: 278-80.

²² Feis, *Churchill, Roosevelt, Stalin*, 64-69.

²³ See Robert H. McNeal, "Roosevelt through Stalin's Spectacles," *International Journal*, 18 (1962-63): 194-206.

²⁴ E. A. Boltin, "Die Wesenszüge der sowjetischen Strategie in der Endphase des Grossen Vaterländischen Krieges," in Bernhard Weissel, ed., *Befreiung und Neubeginn* (Berlin, 1968), 65.

tegic' reasons are fraught with dangerous consequences. . . . Politics and war influence each other but they are not factors of the same order; primacy always belongs to politics."²⁵

The obstacles to a negotiated settlement between the Soviet Union and Germany had diminished by early 1943. Although the passions aroused during the savage struggle ruled out genuine reconciliation, the enormous exertion of both belligerents was conducive to comparing the assets of imperfect peace with the liabilities of elusive victory. Aware of the opportunities, prospective mediators were readily available. Japanese diplomats repeatedly tendered their good services.²⁶ Mussolini, himself in danger of an imminent invasion by the Western Allies, implored Hitler to seek peace with the Russians.²⁷ Among the neutral countries Sweden offered a particularly convenient ambiance for preliminaries. Stockholm, easily accessible from both Moscow and Berlin, was a capital where the members of the opposing coalitions maintained extensive diplomatic and intelligence staffs.

The Soviet legation in Stockholm was headed by the colorful figure of Alexandra M. Kollontay—the aristocratic daughter of a tsarist general, a veteran Bolshevik intellectual of nonconformist leanings, and in her younger days the prophet and practitioner of unconventional views on love, sex, and family.²⁸ In fact these views, rather than diplomatic skill, had qualified her for the foreign service in the early years after the Revolution, when the Bolsheviks delighted in actions *pour épater les bourgeois*. Kollontay was given to emotions in politics and did not hide her Germanophobia even during the period of the official friendship from 1939 to 1941. By 1943 she had reached the age of seventy and was suffering from a heart ailment that confined her to a sanitarium away from the Swedish capital for most of the year. For a historian searching for clues of a Russian-German rapprochement, Kollontay is a great disappointment—a less effective envoy at the sensitive Stockholm post can hardly be imagined. But Stalin's highest-ranking diplomats seldom performed the most important missions. In Sweden, too, officials less colorful but more professional than Kollontay actually ran the legation. Among them was counselor Vladimir S. Semyonov, a specialist in German affairs, whose later career included such elevated functions as chief political adviser of the military government in occupied Germany and eventually deputy foreign minister. Another important official in Stockholm was Boris Yartsev, an expert on Finland, who is on record for having extended peace feelers from Stockholm to Helsinki in

²⁵ E. Razin, "Lenin o sushchnosti voiny" (Lenin on the Nature of War), *Bolshevik*, 19 (1943): no. 1, p. 47.

²⁶ Karl Ott to German foreign office, Mar. 3, 1942; AA, microfilm 39, frames 32999–33002; note on conversation between Ushida and German confidant, Oct. 10, 1942, *ibid.*, frames 33417–18.

²⁷ See F. W. Deakin, *The Brutal Friendship* (New York, 1962), 84–88, 93–94, 102–03.

²⁸ There is a brief, semiofficial biography of Alexandra M. Kollontay by Anna M. Itkina, *Revoliutsioner, Tribun, Diplomat* (Revolutionary, Tribune, Diplomat) (Moscow, 1964), and a sentimental account by Kollontay's friend, Isabel de Palencia, *Alexandra Kollontay: Ambassadress from Russia* (New York, 1947).

late 1942.²⁹ Both Semyonov and Yartsev supposedly maintained a very close relationship with Stalin's security chief, Lavrenti P. Beria, who was known as an advocate of partnership with Germany.³⁰

The German personnel in Stockholm were of equally high caliber, and their selection could be interpreted as a sign of Berlin's interest in a diplomatic settlement of the war. The envoy to Sweden was Hans Thomsen, who had last served as chargé d'affaires in Washington. In addition to Thomsen, in early 1943 Ribbentrop dispatched several respectable diplomats of conservative leanings to other important neutral capitals such as Madrid and Tokyo.³¹ The German foreign minister took pride in having achieved the understanding with the Russians in 1939, and, if his postwar statements are to be trusted, he would have liked to see its renewal in 1943.³² The propaganda minister, Josef Goebbels, was another prominent Nazi favorably disposed to a settlement with Moscow.³³

Thriving on excitement and lucrative connections, volunteers of sometimes dubious character were at hand in wartime Sweden to facilitate liaison with friend and foe. One such individual was a certain Edgar Clauss, a nondescript businessman of Baltic-German ancestry and a temporary resident of Stockholm's Carlton Hotel. He was accompanied by a Swedish lady whom he promised to marry after his allegedly forthcoming but in fact fictitious appointment as German minister in Stockholm. Local Germans thought that he was "either a braggart or a spy," and they warned Thomsen about him.³⁴ As a matter of fact, Clauss was both. Any further inquiry into Clauss by the German legation ceased after German intelligence authorities affirmed that Clauss's activities, which were not specified, served the interests of the Reich.³⁵ But Clauss had evidently more than one iron in the fire. He is the key person in the memoirs of Peter Kleist, who used to travel frequently between Berlin and Stockholm during the war. According to Kleist it was Clauss who told him, on December 14, 1942, that the Russians wanted a separate peace. Alluding to information from the Soviet legation, Clauss said that Moscow was ready to sign an armistice in eight days if only Berlin would respond favorably.³⁶ Although the accuracy of

²⁹ Note on conversation between Ernst von Weizsäcker and Finnish minister to Berlin, Nov. 11, 1942, AA, microfilm 39, frame 33442.

³⁰ Meissner, *Russland, die Westmächte und Deutschland*, 14; James E. McSherry, *Stalin, Hitler and Europe*, 2 (New York, 1970): 17.

³¹ Maxime Mourin, *Les tentatives de paix dans la Seconde Guerre Mondiale* (Paris, 1949), 144-45.

³² Joachim von Ribbentrop, *The Ribbentrop Memoirs*, tr. O. Watson (London, 1954), 170.

³³ Wilfred von Oven, *Mit Goebbels bis zum Ende* (Buenos Aires, 1949), 97-98, 157-59; see also n. 80.

³⁴ Schönwald to German consulate in Stockholm, Mar. 23, 1942, AA, microfilm 719, frames 318871-72.

³⁵ Referred to in Wied, German legation in Stockholm, to German foreign office, May 6, 1942, *ibid.*, frames 318869-70; and Kramarz, German foreign office, to German legation in Stockholm, June 12, 1942, *ibid.*, microfilm 1801, frame E 034410.

³⁶ Kleist, *European Tragedy*, 139-41.

this assertion has not been verified, other Soviet actions at that time give it weight.

On November 6, 1942, shortly before the Red Army encircled the Germans at Stalingrad, Stalin declared in a public speech that "it is not our aim to destroy all organized military force in Germany, for every literate person will understand that this is not only impossible . . . but . . . also inexpedient from the point of view of the victor."³⁷ Stalin had already, on February 23, 1942, publicly refused "to identify Hitler's clique with the German people," suggesting "that Hitlers come and go, but the German people and the German state remain."³⁸ But now he was more specific than ever before in offering friendship to the German military, the caste from which had been recruited so many prominent Russophiles in the past. Following the Treaty of Rapallo in 1922, the German military had been the chief instigators of the deals behind the scenes that enabled them to bypass the restrictions of Versailles by secretly rearming in Russian territory and enabled the Soviet Union in return to obtain German technical assistance. In 1943 the German military were more likely than other Germans to perceive that Hitler's war was lost and to seek salvation in an understanding with Moscow in the spirit of Rapallo.

Shortly after Stalin's address of November 1942 the pattern of Moscow's daily propaganda directed at Germany changed. Until late 1942 the Soviet-sponsored "German People's Radio" had urged in crude Marxist terms that the oppressed German masses intensify the class struggle against their Fascist-capitalist masters.³⁹ By January 1943 a more sophisticated theme had been introduced. The broadcasts began to report a powerful peace movement among Germans regardless of class and political conviction.⁴⁰ On December 6, 1942, a clandestine conference of these Germans for peace was supposed to have met in the Rhineland, and early the following year its proceedings appeared in print in Moscow.⁴¹ Included were speeches by anonymous delegates ranging from Social Democrats to "Christians," organized labor to entrepreneurs, Communists to disillusioned ex-Nazis. But except for sources of Soviet origin, there is no evidence that the meeting ever took place;⁴² in that sense at least, the "peace movement" expressed more the Russian than the German desire for peace.

The Stockholm episode reported by Kleist, Stalin's overture to the putative military opponents of Hitler, and the promotion of the imaginary

³⁷ Stalin, "Twenty-Fifth Anniversary of the Great October Socialist Revolution," Nov. 6, 1942, in Andrew Rothstein, ed. and tr., *Soviet Foreign Policy during the Patriotic War: Documents and Materials* (London, 1944), 1: 49.

³⁸ Stalin, "Order of the Day of the People's Commissar for Defense," Feb. 23, 1942, in *ibid.*, 37.

³⁹ For example, "Appell an das deutsche Volk, unterzeichnet von 158 deutschen Soldaten," in *Sie kämpften für Deutschland* (Berlin, 1959), 114-21; and similar appeals in *Pravda*, Jan. 30, Mar. 19, 1942.

⁴⁰ Report no. A-12777, Oct. 9, 1943, 45621 C, OSS.

⁴¹ *Beratung der nationalen Friedensbewegung in Deutschland* (Moscow, 1943), 12-39.

⁴² See Horst G. Duhnke, "German Communism in the Nazi Era" (Ph.D. dissertation, University of California, Berkeley, 1964), 585-86.

peace front were characteristic products of the peculiar atmosphere created by Stalingrad. They were suggestive of three possible courses that the change of military fortunes opened to the Russians. The first was accommodation with the current Nazi leadership. The second entailed partnership with a conservative German regime dominated by the army. And the third envisaged friendship with a Germany governed by a coalition reminiscent of a popular front but extended to include a wide sampling from the Center and the Right. In due course the Soviet Union was to explore all three of these options.

STALINGRAD INAUGURATED the most intriguing period of Soviet wartime diplomacy. The great battle brought a crushing defeat to Hitler but did not strengthen the solidarity of his enemies. In fact the Russians began almost immediately to put that solidarity to test. They harassed the British personnel attached to the arms convoys, interfering with their navigation and subjecting them to various indignities.⁴³ The official Soviet press was conspicuously deficient in acknowledging American lend-lease deliveries, a deficiency that prompted public criticism by the United States ambassador, William H. Standley, and an angry Russian reaction in return.⁴⁴ Most important, in both his public and his confidential statements Stalin indicated doubts about the suitability of the Allied coalition as a vehicle for his interests.

On February 23, 1943, Stalin's address on the Day of the Red Army did not even mention the Allies.⁴⁵ It presented the war as an exclusively Soviet-German affair. Stalin's words were not calculated to reassure Western military planners, many of whom feared that the Russians would stop fighting once they reached their prewar frontiers. Indeed the sharp curtailment of offensive operations by the Red Army on March 15, 1943, tended to confirm rather than to destroy that disturbing hypothesis.⁴⁶ The unprecedented calm that afterwards prevailed on the battlefields of the eastern front was perhaps justified by the great exertions of the recent months. But the beginning of the calm also coincided to a day with Stalin's "most emphatic warnings" in a reply to Churchill's notification of further obstacles to an early landing in Europe. The message from Moscow referred to "the great danger with which further delay in opening a second front in France

⁴³ S. W. Roskill, *The War at Sea, 1939-45*, 2 (London, 1950): 400-01; Woodward, *British Foreign Policy*, 2: 566.

⁴⁴ William H. Standley and Arthur A. Ageton, *Admiral Ambassador to Russia* (Chicago, 1955), 341-42.

⁴⁵ Stalin, "Order of the Day of the Red Army," Feb. 23, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 53-57. See also Charles Bohlen, memorandum, Feb. 23, 1943, and Standley to Secretary of State, Feb. 24, 1943, *FRUS, 1943*, 3: *The British Commonwealth, Eastern Europe, the Far East* (Washington, 1963), 506-09.

⁴⁶ See Earl F. Ziemke, *Stalingrad to Berlin: The German Defeat in the East* (Washington, 1968), 118, 128-31.

is fraught.”⁴⁷ In his efforts to accelerate the opening of the second front Stalin had issued similar warnings before. But this time the military lull, as well as the intensive peace propaganda on the “German People’s Radio,” gave his words a special air of urgency. The Soviet Union was demonstrating that its attitude toward Germany was not the same as that of the Western Allies. In particular it was scrupulously avoiding any association with the demand for unconditional surrender that Roosevelt and Churchill had enunciated during their conference at Casablanca in January 1943.⁴⁸

So disconcerting was the behavior of the Russians that at the end of February the British government instructed its ambassador to Moscow, Sir Archibald Clark-Kerr, to inquire on the spot what Soviet intentions with Germany really were. The envoy contacted both Molotov and Stalin, but the “reply was not in very friendly terms.”⁴⁹ The Soviet leaders evidently did not wish to prejudice their own course of action. For the moment there were still too many open questions that only time could sort out. But having had more firsthand experience with the power and determination of the enemy than the British and Americans, the Russians definitely entertained fewer illusions about the feasibility of a permanent suppression of Germany.⁵⁰ They preferred an indirect solution to their security problem by seeking a zone of dependent states as protection against any future attack. Poland, not Germany, was therefore the most urgent item on their agenda.

The dramatic deterioration of relations between Moscow and the Polish government in exile during the first four months of 1943 was more of Russian than of Polish making. The Soviet Union took the first decisive steps after reiterating its claims to the disputed borderland. On February 16 the Soviet deputy foreign commissar, Alexander E. Korneichuk, took a strong public stand against the Polish representatives in London because of their unwillingness to grant those claims.⁵¹ And on March 1 the Russians launched the Union of Polish Patriots, an organization that could later serve as the nucleus of a puppet government.

These Soviet actions predictably caused distress in Washington and Lon-

⁴⁷ Churchill to Stalin, Mar. 11, 1943; Stalin to Churchill, Mar. 15, 1943, Ministry of Foreign Affairs of the U.S.S.R., ed. and tr., *Stalin's Correspondence with Churchill and Attlee* (New York, 1965), 106, 99–102.

⁴⁸ See Anne Armstrong, *Unconditional Surrender* (New Brunswick, 1961), 55–58; and John L. Chase, “Unconditional Surrender Reconsidered,” *Political Science Quarterly*, 70 (1955): 258–79.

⁴⁹ Referred to in Woodward, *British Foreign Policy*, 2: 552.

⁵⁰ There is still no satisfactory study about the development of the wartime Soviet policy on Germany. In addition to Duhnke's dissertation on “German Communism in the Nazi Era,” the most useful studies are Karl-Heinz Ruffmann, “Das Gewicht Deutschlands in der sowjetischen Aussenpolitik bis zum Ende des Zweiten Weltkrieges,” *Aus Politik und Zeitgeschichte* (1970), no. 2, pp. 3–18; Alexander Fischer, “Antifaschismus und Demokratie: Zur Deutschlandplanung der UdSSR in den Jahren 1943–1945,” in *Potsdam und die deutsche Frage* (Cologne, 1970), 6–33; and Ernst Deuerlein, “Das Problem der ‘Behandlung Deutschlands’: Umrisse eines Schlagwortes des Epochenjahres 1945,” *Aus Politik und Zeitgeschichte* (1965), no. 8, pp. 26–46.

⁵¹ Alexander E. Korneichuk, “Vossoedenie ukrainskogo naroda v nedrach svoego gossudarstva” (Reunion of the Ukrainian People in the Bosom of Its State), *Pravda*, Feb. 20, 1943. See also TASS statement, Mar. 3, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 264–65.

don, though not in Berlin. Moscow's insistence upon the frontier that Molotov had fixed with Ribbentrop in 1939 in fact brought back memories to the Germans of their former intimacy with Russia. But so great was the Nazis' intransigence, as well as their lack of subtlety, that they disregarded these friendly allusions. They rather chose to inflict upon the Soviet Union a major irritation. On April 13 they announced the discovery at Katyn of mass graves of murdered Polish officers and accused Moscow of responsibility for the crime. Whatever Goebbels's disposition in favor of an understanding with the Russians, he proved unable to resist this exceptional opportunity to embarrass them.

The genuine dismay and studied indignation displayed by the Soviet government, almost certainly responsible for the massacre, are hardly surprising.⁵² More suggestive are the limits and the direction of Soviet anger. In an initial impulse to gloss over the German indiscretion the official Russian news bulletin hinted at the rather improbable explanation that the Nazis might have mistaken graves for archeological excavations.⁵³ Only when the Germans continued to insist upon their version of what had happened at Katyn did Moscow accuse them, without reservation, of the killings. At the same time, however, the Soviet government tried to divert attention from the merit of the German case in a fashion that could only give additional comfort to the Nazis.⁵⁴ After the Poles had proposed investigation by the International Red Cross, a proposal that Berlin promptly implemented, Moscow added insult to injury by accusing the Poles of collusion with Hitler. Worse still, the Soviet Union on April 26 broke off diplomatic relations with Poland, thus bringing the crisis to a climax.

Stalin's handling of the Katyn affair did not bar a rapprochement with Germany and may have actually served to facilitate it. There are indications of secret approaches at the very time the two belligerents were exchanging public insults. The information available is independent of that from Kleist, who did not happen to be in Stockholm in that particular period. A document in the United States Army Intelligence files mentions a communication in mid-April from Moscow to the French Communists, alerting them to be prepared for possible armistice talks.⁵⁵ And according to Swedish informants of the American Office of Strategic Services, such talks took place shortly thereafter.⁵⁶ In the latter part of April a Swede with connections at the Russian legation was said to have arranged a meeting of

⁵² Janusz K. Zawodny summarizes the evidence in *Death in the Forest* (Notre Dame, 1962), 77-99.

⁵³ Bulletin of the Soviet radio, Apr. 15, 1943, in *Zbrodnia katyńska w świetle dokumentów* (The Katyn Crime in the Light of Documents) (London, 1948), 104.

⁵⁴ "Polskie sotrudniki Gitlera" (Polish Collaborators of Hitler), *Pravda*, Apr. 19, 1943.

⁵⁵ OSS report no. A-5094, May 11, 1943, USSR 3700, G-2. See also "The Problem of a Separate Peace between Germany and Russia," enclosed in A. J. Drexel Biddle to Secretary of State, June 26, 1943, 740.00119 EW/1530, DS.

⁵⁶ OSS report no. A-9469, Aug. 9, 1943, USSR 6900, G-2. See also Polish intelligence report no. 1297, early May 1943, *ibid*.

diplomats at a country estate about thirty kilometers outside of Stockholm. The participants included three unnamed German officials and, from the Soviet side, Mikhail Nikitin, Alexei Taradin, and Boris Yartsev. At one time the respective ministers, Thomsen and Kollontay, were said to have joined the discussions, which lasted for several days but did not lead to an agreement. The Germans were supposedly ready to make peace in return for a satellite Ukraine and for economic concessions in other parts of the Soviet Union, whereas the Russians insisted upon the frontier of 1941.⁵⁷

This intelligence report deserves respect as the most specific contemporary piece of information about the peace feelers. Although there is no way of checking the accuracy of its details, it is quite probable that informal exchanges occurred at Soviet initiative. The pattern of Moscow's policy after Stalingrad supports rather than contradicts this speculation. It was in the spring of 1943, if ever during the war, that the time was ripe for Stalin to explore whether Berlin would be prepared to settle for what his Western allies had been so reluctant to grant him—the confirmation of the Soviet frontier of 1941. But the attitude of the Germans was altogether different from what it had been in August 1939, and they gave no sign of any willingness to restore the frontier they had violated in June 1941.

The Russian participants reportedly broke off the negotiations in Sweden at the beginning of May. In his Order of the Day on May 1 Stalin had publicly assumed a position on the subject of separate peace. He attributed the desire for a separate peace to the Nazis, who “judge their adversaries by their own standards of treachery.”⁵⁸ He also affirmed that only unconditional surrender of the enemy could end the war, although he did not relate this demand to the Casablanca statement by Roosevelt and Churchill. Stalin's strong language suggests that he may have given up any hope of persuading the obstinate Nazis that a compromise was not only in his interest but, needless to say, also in theirs.

But there is another possible interpretation of what Stalin was saying. His statement could have been meant as an encouragement for the renewal of talks on terms more acceptable to him. The triangular nature of the Soviet–Western-German relationship was obvious, and he could alternately show a friendly face to one side or the other. His most convincing argument would have been to demonstrate to the Germans that he could obtain what he wanted from the West. From this point of view, the rather meek reaction of the British and Americans to the Katyn incident gave the impression that Stalin might be able to repair his relations with

⁵⁷ These terms are not mentioned specifically in relation to the April talks, but they recur in intelligence reports from late 1942 to the fall of 1943. See, for example, London SI report, Nov. 3, 1942, USSR 3850, G-2; OSS report no. A-1820, Feb. 5, 1943, USSR 6900, G-2. See also Huene, Lisbon, to German foreign office, Apr. 6, 1943, AA, microfilm 39, frames 33667–68; OSS report no. A-13022-b, Oct. 13, 1943, USSR 6900, G-2; and Kleist, *European Tragedy*, 140.

⁵⁸ Stalin, “Order of the Day of the Supreme Commander-in-Chief,” May 1, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 58.

his coalition partners whenever he wanted. They had given proof of an overwhelming desire to maintain at least the appearances of Allied solidarity. And it was the appearance, though not the substance, of this solidarity that the Russians proceeded to promote during the next few weeks.

STALIN'S ADDRESS of May 1, 1943, heralded a conspicuous improvement in official Soviet behavior toward Great Britain and the United States, a difference the more striking since no particular change in the conduct of the Western Allies had prompted it. Alexander Werth, who lived in Moscow as correspondent for the *Sunday Times*, later recalled "the record warmth vis-à-vis Britain and America in May and June 1943."⁵⁹ The official press and radio prominently commemorated the anniversaries of the alliance agreements of 1942 and extolled the Allied operations in North Africa, a battlefield previously denigrated as insignificant.⁶⁰ Russian editorial writers ridiculed the idea of a separate peace and seconded Stalin's call for the unconditional surrender of Germany. And on May 23 the dissolution of the Comintern generated further good feelings in the West.

Yet no practical steps toward closer collaboration accompanied these friendly gestures. In particular, Stalin remained aloof to Roosevelt's urgent pleas for a tripartite summit conference that would clarify mutual war and peace goals.⁶¹ It is difficult to judge to what extent Stalin's aloofness was the result of premeditation rather than of a genuine need to keep a close eye on the developments at the front—the explanation he gave to the president. As Western observers in Moscow noticed, the Russians were at that time especially nervous about the intentions of Germany.⁶² On the one hand, the inexplicable delay of the anticipated and dreaded German summer offensive could mean that Berlin might be contemplating negotiations after all. On the other hand, however, as long as there was no positive evidence of such a readiness, Stalin had to assume that the offensive would eventually come and that he would need whatever relief his allies could give him. Whether to prompt them into action or simply to cheer up the Soviet people, the official press systematically encouraged the belief that now, after the victory in Africa, the second front was imminent.

It is against this background of inflated Russian hopes that the impact of yet another message about a delay of the cross-Channel invasion must be measured. On June 4 Roosevelt and Churchill notified their Soviet

⁵⁹ Alexander Werth, *Russia at War, 1941-1945* (New York, 1964), 671-72.

⁶⁰ "Godovshchina sovetско-angliiskogo soiuзнного dogovora" (The Anniversary of the Soviet-English Treaty of Alliance), *Pravda*, May 26, 1943; "Koalitsiia rozhdennaia voinoi i prizvannaia obespechit pobedonosnyi mir" (A Coalition Born of War and Destined to Secure a Victorious Peace), *Voina i rabochii klass*, June 15, 1943, pp. 3-9.

⁶¹ Roosevelt to Stalin, May 5, 1943; Stalin to Roosevelt, May 26, 1943, Ministry of Foreign Affairs of the U.S.S.R., ed. and tr., *Stalin's Correspondence with Roosevelt and Truman* (New York, 1965), 63-64, 66.

⁶² Werth, *Russia at War*, 674-76.

colleague that the operation could not take place before May 1944.⁶³ Stalin's fury at this new apparent procrastination at a time so critical for him is easy to imagine and justify. But as during the Katyn crisis, his reaction was reasoned rather than impulsive. He waited for a whole week, undoubtedly weighing alternative responses and considering their probable effect. In the end he expressed his intense displeasure not only in a secret message but also through an important public gesture. He recalled almost simultaneously the reputedly pro-Western ambassadors Ivan M. Maisky and Maxim Litvinov from their respective posts in London and Washington.

By mid-June, then, the brief period of Moscow's official amity with the West was over. There is no doubt that the Roosevelt-Churchill action—or rather inaction—about the second front had preconditioned the new crisis, but it was Stalin's public behavior that actually created it. He could hardly have failed to take into account the delight that evidence of Allied discord would inevitably produce in Berlin, and he may have aimed at that very effect. In any case, he again set the stage for a rapprochement with Germany, and there are indications of Russian attempts to promote it in the second half of June. On June 16 the Swedish newspaper *Nya Dagligt Allehanda* announced in a special edition that high Soviet and German officials had been negotiating near Stockholm.⁶⁴ An American intelligence report identified them as Mikhail Nikitin of the Russian legation and Paul Schmidt of the press and information section of the German foreign office, and the seaside resort of Saltsjöbaden was cited as their meeting place.⁶⁵ The British government, too, received information from the Swedish capital that Schmidt had met with two members of the Soviet legation in a private house and that Yartsev played the key role in the contact.⁶⁶ According to Kleist's somewhat different account, A. M. Alexandrov, formerly counselor at the Soviet Embassy in Berlin and head of the foreign commissariat's European division, arrived in Stockholm en route to London and tried to contact him through Clauss.⁶⁷ The German minister, Thomsen, also heard that the Russian diplomat was in town and that he wanted "to meet with a gentleman from the German foreign service with whom he was acquainted."⁶⁸

⁶³ Roosevelt to Stalin, received June 4, 1943; Stalin to Roosevelt, June 11, 1943, *Stalin's Correspondence with Roosevelt and Truman*, 67–71.

⁶⁴ Reported in *New York Times*, June 17, 1943. A slightly different version appeared in the Geneva newspaper *La Suisse* on Oct. 2, 1943.

⁶⁵ OSS report no. A-9647, Aug. 9, 1943, USSR 6900, G-2; Herschel Johnson, Stockholm, to Secretary of State, June 17, 1943, *ibid.*

⁶⁶ George Wiskeman to Christopher Warner, British Foreign Office, Aug. 11, 23, 1943, N 4898/66/38, FO 371/36956.

⁶⁷ Kleist, *European Tragedy*, 144–50.

⁶⁸ Thomsen, Stockholm, to German foreign office, June 21, 1943, AA, microfilm 191, frames 144408–09. Basil H. Liddell-Hart, the noted British military historian who had interrogated captured German officers, claimed in his posthumously published book that negotiators, including even Ribbentrop and Molotov, had met behind the German lines near Kirovograd in June 1943. *History of the Second World War* (New York, 1970), 488. Because of the complete lack of supporting evidence, however, this allegation deserves little credence.

After the war this incident received a retrospective publicity in the American press. In July 1947 an article in the New York magazine *Liberty* added further sensational details, which the authors claimed they had acquired during "clandestine conferences with Europeans whose lives depend on anonymity."⁶⁹ It is interesting that the Soviet government considered this rather shabby piece of writing worthy of a prompt response. No one less authoritative than Kollontay herself was commissioned to refute the allegations. In a column printed in *Izvestiia* she concentrated upon a single flaw in the *Liberty* article: Alexandrov could not possibly have been in Stockholm at the critical time because he was serving with the Soviet legation in Australia.⁷⁰ But the Russian sensitivity about the subject enhances rather than reduces the possibility that Alexandrov's colleagues from the foreign commissariat had indeed tried to extend feelers in Stockholm, even though an early indiscretion apparently prevented actual contacts. Such a speculation would also explain why both the Soviet and the German governments had denied the original Swedish newspaper story so vehemently in press communiqués issued on the same day, June 18, 1943.⁷¹

But even after the embarrassment in June Moscow did not abstain from publicizing ideas suggestive of a preference for compromise over struggle to the bitter end. On June 22, 1943, the official information agency Sovinformburo commented ambiguously that "without a second front victory over Germany is impossible."⁷² On July 1 an authoritative article in the ideological journal *Voina i rabochii klass* castigated Western projects for the postwar punishment of the Germans.⁷³ Its author, identified as N. Malinin, derided the theory about the collective guilt of the German people and expressed misgivings about subjecting them to reparations or even to a military occupation. Of all Soviet wartime statements this article went the farthest in hinting that the Germans might be allowed to keep some of their conquests. Questioning not only Polish but also Czechoslovak aspirations for territories that were currently part of the Reich, the author implied that even the return of the Sudetenland to Czechoslovakia was not imperative. Exactly to whom Moscow intended to address these extraordinary allurements is not altogether clear. Stalin may still have hoped that the Nazis could be persuaded to negotiate, but if he did, the advent of the German offensive on July 5 all but destroyed any such hopes. It is more likely that the article foreshadowed a change of policy that had been under preparation ever since the demise of the Comintern and for which

⁶⁹ Paul Schwarz and Guy Richards, "A Secret Russian Mission That Almost Changed History," *Liberty*, July 5, 1947, p. 26. See also similar articles by Donald B. Sanders [pseud.], "Stalin Plotted a Separate Peace," *American Mercury*, Nov. 1947, pp. 519-27; and Robert M. W. Kempner, "Stalin's 'Separate Peace' in 1943," *United Nations World*, 4 (1950): no. 3, pp. 7-9.

⁷⁰ *Izvestiia*, July 29, 1947.

⁷¹ TASS statement, June 18, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 269; *New York Times*, June 18, 1943.

⁷² "Dva goda Otechestvennoi voiny Sovetskogo soiuza" (Two Years of the Patriotic War of the Soviet Union), *Pravda*, June 22, 1943.

⁷³ N. Malinin, "O tseliakh voiny" (Concerning the War Aims), *Voina i rabochii klass*, July 1, 1943, pp. 11-15.

the enemy attack provided the appropriate moment.⁷⁴ The formation of the Free Germany Committee on July 12 marked the beginning of the new Soviet strategy.⁷⁵

Retrospective interpretations have tended to obscure the original meaning of this remarkable enterprise. Both friendly and hostile commentators have been inclined to regard the formation of the Free Germany Committee as the first step on the path that ended with the establishment of the German Communist satellite state in 1949.⁷⁶ But evidence that the Russians anticipated this outcome in 1943 is yet to be presented. Most contemporaries understood Russian sponsorship of the committee as a bid for partnership with German conservatives, and the Soviet Union did nothing to discourage this interpretation.⁷⁷ Although Communist émigrés predictably dominated the committee, its striking feature was the participation of the more moderate patriotic members who had been recruited from among the prisoners of war. Their radio appeals, urging the German army to end the war by overthrowing Hitler and withdrawing to the present frontier of the Reich, implied that this relatively modest concession would make possible a fair peace. Whether or not the Russians would have honored this implicit promise is impossible to judge. Yet by having given their blessing to the Free Germany movement they created an obstacle to a rapprochement with Hitler; they invited an understanding instead with his potential successors.

THE SOVIET VICTORY in the battle of Kursk, which ended on July 16, represents in many ways a watershed more important than Stalingrad in the history of the war. The first summertime triumph of Russian arms, it opened the perspective of their continuous and irreversible, though still far from easy, advance to the west. The Germans saw the handwriting on the wall, and doubts about the wisdom of fighting on extended this time to the highest places. Various Nazi officials, acting on their own, tried to find out about Moscow's possible terms for peace. On August 2–5, 1943, Ribbentrop's assistant Rudolf Likus went to Stockholm in order to gather information.⁷⁸ Two weeks later the German foreign minister sum-

⁷⁴ Communist Party of Germany, Directive of May 27, 1943, referred to in Bruno Löwel, "Die Gründung des NKFD im Lichte der Entwicklung der Strategie und Taktik der KPD," *Beiträge zur Geschichte der deutschen Arbeiterbewegung*, 5 (1963): 618–19.

⁷⁵ "Manifest des Nationalkomitees 'Freies Deutschland' an die Wehrmacht und an das deutsche Volk," July 12–13, 1943, in *Sie kämpften für Deutschland*, 146–51. The best history of the committee is Bodo Scheurig, *Free Germany* (Middletown, Conn., 1970).

⁷⁶ Representative samples of the opposing views are Gerhard Rossmann, "Die Entwicklung der Vorstellungen der KPD über den neuen Staat," *Beiträge zur Geschichte der deutschen Arbeiterbewegung*, 9 (1969): special no., pp. 145–62; and Peter Strassner, *Verräter: Das Nationalkomitee "Freies Deutschland"—Keimzelle der sogenannten DDR* (Munich, 1960).

⁷⁷ One of the best contemporary assessments is Arnold Wolfers, "Soviet Policy toward Germany," R & A Branch, 66834 R, OSS. For a different view, see the article by German émigré Egon Kötting [Eugen Westphal], "Nationalbolsjevism—Tysklands Framtid?" (National Bolshevism as Germany's Future?), *Svensk Tidskrift*, 30 (1943): 496–505.

⁷⁸ Rudolf Likus, note, Aug. 9, 1943, records of the German foreign office, AA, microfilm 162, frames 130779–80. See also Walter Schellenberg, *The Labyrinth* (New York, 1956), 370.

moned Kleist and instructed him to restore contact with Clauss.⁷⁹ In September Goebbels told Hitler that "we must come to an arrangement with one side or the other," and the Führer replied that "he would prefer negotiations with Stalin," although Hitler did not "believe they would be successful."⁸⁰

All these half-hearted peace stirrings on the Nazi side ended in the planning stage. They sufficed, however, to cause grave concern among the men around Karl Goerdeler who hoped to extricate Germany from the war by getting rid of Hitler. One of the conspirators, Ulrich von Hassell, noted in his diary on August 15, 1943:

If Hitler comes to terms with Stalin, the resultant disaster cannot be imagined. It would be different with a decent, self-respecting Germany. This Germany would have to exploit all opportunities. There is only one expedient left—to make either Russia or the Anglo-Americans understand their interest in a sound Germany.⁸¹

Hassell, like most of his friends, would have preferred settlement with the Western powers if only they would modify their stiff demand for unconditional surrender. But at least a few members of the conspiracy favored approaches to the Soviet Union, perhaps because they hoped to find it more responsive or because they considered communism the wave of the future. In the late summer of 1943 Friedrich von der Schulenburg, the former ambassador to Russia, contemplated a secret mission to Moscow by breaking through the front lines.⁸² There is also evidence, however inconclusive, that communist sympathies may have motivated the would-be assassin of Hitler, Claus von Stauffenberg.⁸³

Did the Russians know about the plot against Hitler and attune their policies to the possibility of its success? From what is known about their intelligence network, it seems that until 1942 they had been informed quite accurately about what was happening in Germany. Their main source of information had been the famous espionage organization known as the Red Orchestra.⁸⁴ Although the standard East German book about the Red Orchestra claims that its members knew of the activities of Goerdeler's followers, it offers no specific evidence in support of this claim.⁸⁵ In

⁷⁹ Kleist, *European Tragedy*, 154–56.

⁸⁰ Goebbels, diary entry for Sept. 23, 1943, *The Goebbels Diaries*, tr. L. P. Lochner (Garden City, 1948), 477.

⁸¹ Hassell, diary entry for Aug. 15, 1943, *The Von Hassell Diaries*, tr. H. Gibson (Garden City, 1947), 315.

⁸² Ernst Kaltenbrunner to Martin Bormann, Aug. 28, 1944, in Kaltenbrunner, *Spiegelbild einer Verschwörung* (Stuttgart, 1961), 308–09. See also Hassell, diary entry for Dec. 5, 1943, *Von Hassell Diaries*, 327.

⁸³ See Hans Bernd Gisevius, *To the Bitter End* (Boston, 1947), 486; and Joachim Kramarz, *Claus Graf Stauffenberg* (Frankfurt, 1965), 175–77, 192–93. See also Hans Dress, "Fortschrittliche und reaktionäre Tendenzen in den Reformplänen des Kreisauer Kreises," in Kommission der Historiker der DDR und UdSSR, ed., *Der deutsche Imperialismus und der zweite Weltkrieg* (Berlin, 1961), 4: 587–606.

⁸⁴ The best book on the subject is Heinz Höhne, *Codeword DIREKTOR: The Story of the Red Orchestra* (New York, 1972).

⁸⁵ Karl-Heinz Biernat and Luise Kraushaar, *Die Schulze-Boysen/Harnack-Organisation im antifaschistischen Kampf* (Berlin, 1970), 28.

any case the Gestapo had destroyed that principal Soviet source of information by the fall of 1942.⁸⁶ In the trial in April 1944 of William Knöchel, a high-ranking German Communist whom Moscow had sent to Germany in January 1942 in order to organize the party underground, the Nazi court noted that before Knöchel's capture later in 1942 he had sent abroad important information about opposition currents in the country.⁸⁷ But this information must have been rather unsubstantial since the knowledge of it did not enable the Gestapo to track down the conspiracy.

As for 1943, Russian and East German literature, which normally plays up rather than down all types of clandestine pro-Soviet activities, has recorded only one instance of a line of communication between Berlin and Moscow in existence during that year. The exchanges began in spring, when Soviet confidants in Stockholm established contact, through a Swedish intermediary, with Anton Saefkow, a leading Communist organizer in Berlin. Saefkow is known to have met two associates of Goerdeler in June 1944, and Saefkow was aware of the plot against Hitler's life possibly as early as the end of 1943.⁸⁸ Saefkow may have notified the Russians of the plot, although available sources give the impression that little of substance passed through this Swedish channel.⁸⁹ Surviving participants in the conspiracy have all denied that any links with Moscow ever existed.⁹⁰ It is therefore unlikely that the Soviet anticipation of a revolt in Germany was anything but an intelligent guess, made without the benefit of knowing what was actually under way.

Just how great the Russians thought were the chances of a successful coup against Hitler is difficult to estimate. But its likelihood definitely increased after the Italians had set an example by deposing Mussolini on July 25, 1943. Since the change of government in Italy was accomplished by the ruling clique rather than by the masses, Moscow had a further reason to focus upon fomenting discontent among Germany's upper classes. Shortly after the Italian events the Soviet Union prepared to supplement the Free Germany Committee, on which Walter Ulbricht and his companions loomed perhaps too large, with the more respectable and exclusive League of German Officers. In the meantime the Russian victory at Kursk had convinced so many captured Germans that the war was lost that there was no shortage of those willing to join in this new enterprise.

Wolfgang Leonhard, who worked during the war in the Moscow offices

⁸⁶ David J. Dallin, *Soviet Espionage* (New Haven, 1955), 264-65.

⁸⁷ Arnold Sywottek, *Deutsche Volksdemokratie: Studien zur politischen Konzeption der KPD 1935-1946* (Düsseldorf, 1971), 118, 245.

⁸⁸ Gerhard Rossmann, *Der Kampf der KPD um die Einheit aller Hitlergegner* (Berlin, 1963), 45-47, 195-96, 222, 255; Gertrud Glondajewski and Gerhard Rossmann, "Ein bedeutendes politisches Dokument des illegalen antifaschistischen Kampfes der Kommunistischen Partei Deutschlands," *Beiträge zur Geschichte der deutschen Arbeiterbewegung*, 8 (1966): 646-47; Allen W. Dulles, *Germany's Underground* (New York, 1947), 173-74.

⁸⁹ See Rossmann, *Der Kampf der KPD*, 45-47; and Otto Winzer, *Zwölf Jahre Kampf gegen Faschismus und Krieg* (Düsseldorf, 1955), 231.

⁹⁰ See Bodo Scheurig, ed., *Ferrat hinter Stacheldraht?* (Munich, 1965), 232-35.

of the journal *Freies Deutschland*, later reported that the inauguration of the League of German Officers, set for September 1, had been unexpectedly postponed without a substitute date.⁹¹ A few days later the journal was about to publish a startling editorial entitled "Armistice—the Demand of the Day." According to Leonhard this editorial, which omitted the customary anti-Nazi verbiage, was addressed to the men currently in power in Berlin rather than to their prospective challengers. It was withheld from publication only at the last moment, and the delayed proclamation of the league followed on September 11.⁹²

Leonhard's assertion seems to corroborate Kleist's testimony that on September 8 the Russians again attempted to establish contacts in Sweden through Clauss.⁹³ The intermediary alerted Kleist that Vladimir Dekanozov, the former Soviet ambassador to Berlin and another of Beria's protégés, was going to visit Stockholm and was eager to meet a German negotiator. According to Clauss, Moscow merely awaited a signal from Berlin and was deeply disappointed when none came.

The coincidence of Leonhard's and Kleist's allegations is well enough known and has led to the belief that Stalin may have made a major bid for peace in September rather than in April or June 1943.⁹⁴ Such a conclusion is possible but not probable; for aside from the evidence previously presented of Nazi unresponsiveness, Kursk had bolstered the self-confidence of the Russians sufficiently to reduce for them the value of any deal with Hitler. The hypothesis also depends much too heavily on the veracity of the disreputable Clauss, who very likely misled both the German and the Soviet diplomats about the actual extent of his intimacy with each of them.

A more probable explanation of what happened during those first few days of September is that the presence of Ribbentrop's emissary in Stockholm, along with Clauss's grandiloquence, almost convinced the Russians that Berlin had unexpectedly changed its mind about negotiating. Then the article in *Freies Deutschland* would have been a tentative sign of their interest. But the hoax was exposed just in time to spare the Russians the same embarrassment they had experienced in June, when the Swedish newspaper had leaked similar shady dealings. If this was the case, then the Soviet government, though no longer itself trying to extend peace feelers to the present German regime, was in early September 1943 still willing to respond to initiatives by others.

Soon, however, even that willingness disappeared, and the experience with Clauss may have been the last blow. On September 13 Molotov re-

⁹¹ Wolfgang Leonhard, *Child of the Revolution* (Chicago, 1958), 318–20.

⁹² Scheurig, *Free Germany*, 65. See also Heinrich von Einsiedel, *I Joined the Russians* (New Haven, 1953), 98–106.

⁹³ Kleist, *European Tragedy*, 164–71.

⁹⁴ See, for example, Minuth, "Sowjetisch-deutsche Friedenskontakte," 38–45; and Lionel Kochan, *The Struggle for Germany* (New York, 1967), 96.

jected a mediation offer by the Japanese ambassador, Naotake Sato, intimating to him that whatever chances there had been of an understanding between Germany and Russia no longer existed. Molotov is quoted as having said: "Under different circumstances, the Soviet government would have considered it its duty to accept the Japanese offer of mediation."⁹⁵

The rules of the diplomatic triangle now required that Stalin should attempt to improve his relations with the British and the Americans. And indeed, from September onwards, he acted to instill more life into the alliance—not only in words but also in deeds. The decisive date was September 8, when, after months of dilatory excuses, he finally consented to a conference with Roosevelt and Churchill and accepted the date they had proposed.⁹⁶ A week later Stalin proved that he cared more about winning their confidence than about keeping open any secret channels to the enemy: Andrei Gromyko, chargé d'affaires in Washington, duly reported the Japanese mediation offer to the secretary of state, Cordell Hull.⁹⁷ Equally significant, Konstantin Vinogradov of the Soviet legation in Stockholm revealed to an American colleague that "German agents and intermediaries" had recently approached Russian diplomats there—a disclosure in striking contrast to his consistent denials of such approaches in the past.⁹⁸

By the fall of 1943 evidence had mounted that a peace with Hitler was impossible and his replacement by more reasonable men improbable. The Russians nevertheless abstained from endorsing the formula for unconditional surrender until after the Normandy landings in June 1944.⁹⁹ As long as the second front was in abeyance they avoided doing anything that would prejudice a separate arrangement with an anti-Nazi regime in case Hitler's enemies would come to power after all. But after September 1943 the Soviet Union no longer went so far as actively to prepare for such an arrangement.

THE OBVIOUS QUESTION to be asked is whether Stalin did not himself try to create a false impression of his readiness to conclude a separate peace merely to make his allies more amenable to his demands. After the war many Western authors replied in the affirmative. General Albert C. Wedemeyer, for example, concluded that the Soviet dictator "was simply holding the idea over our heads and blackmailing us to give him more aid."¹⁰⁰

⁹⁵ Quoted in Toshikazu Kase, *Journey to the Missouri* (New Haven, 1950), 162–63.

⁹⁶ Stalin to Roosevelt, Sept. 8, 1943, *Stalin's Correspondence with Roosevelt and Truman*, 90–91.

⁹⁷ Gromyko to Hull, Sept. 14, delivered Sept. 16, 1943, *FRUS*, 1943, 3: 696–97.

⁹⁸ Johnson, Stockholm, to Secretary of State, Sept. 29, 1943, *ibid.*, 698–99.

⁹⁹ After the Russian delegates had used delaying tactics in the European Advisory Commission for six months, they approved its draft instrument for German surrender on July 25, 1944. *FRUS*, 1944, 1: *General* (Washington, 1966), 252–54. The chief Soviet representative, Fedor Gusev, announced the confirmation of the document by his government on August 21, 1944. John G. Winant to Secretary of State, Aug. 21, 1944, *ibid.*, 276.

¹⁰⁰ Albert C. Wedemeyer, *Wedemeyer Reports!* (New York, 1958), 92.

To attribute to the Russians a genuine desire for compromise with anyone would not have been compatible with the belief in their insatiable hunger for power, a belief prevalent during the cold war.

But Stalin's aptitude at deception notwithstanding, his alleged scheme to frighten his coalition partners cannot be reconciled with the evidence. Peace rumors emanated from so many different sources, independent of one another, that they could not have possibly been disseminated from one center.¹⁰¹ In addition Stalin had strong enough reasons to refrain from mystification: planting false reports about secret dealings with the enemy could only increase the chances that the Western capitalists and their German colleagues might turn the tables on him. Accordingly the Russians coped with the many circulating rumors in a fashion calculated to discourage speculation rather than to encourage it. They ignored those rumors that were too vague or too fantastic to be taken seriously and reacted only to the ones with substance: the three abortive feelers in Stockholm and the Japanese mediation attempts were the incidents that prompted official refutations.¹⁰² Thus the Soviet government tried to divert attention from what had happened rather than attract it to what had not. Those contemporaries who thought that rumors were being deliberately planted suspected a German scheme much more often than a Russian one.¹⁰³ But the Nazis, though no doubt eager to sow mistrust among their adversaries, were also apprehensive that such a maneuver might backfire against their own national morale. They therefore frequently denied that any peace efforts were under way, using for that purpose the foreign service of the German radio rather than the domestic news media.¹⁰⁴

Rumors of peace feelers proliferated throughout the summer and fall of 1943 despite Russian and German efforts to suppress them. They were especially rife in Latin America, where amateurs and imposters, within and without the diplomatic corps, disseminated the most sensational stories. On September 28, 1943, for example, the newspapers in Arequipa, Peru's second largest city, announced that an armistice between Germany and Russia had just been signed.¹⁰⁵ In Switzerland there was much talk about mediation by the Vatican.¹⁰⁶ And various Eastern European sources re-

¹⁰¹ See memorandum by Harvey H. Smith, Chief of G-2 Central European Branch, Sept. 20, 1943, Germany 6900, G-2.

¹⁰² See nn. 58, 71, 97, 98; and TASS statement, July 17, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 269. A Japanese mediation attempt in June 1943 is mentioned in Ivan Krylov [pseud.], *Soviet Staff Officer* (London, 1951), 241-42.

¹⁰³ Navy intelligence report, no. 660-43, Sept. 26, 1943, USSR 6900, G-2; information series no. 103A, Nov. 15, 1943, Germany 6900, *ibid*.

¹⁰⁴ Broadcasting monitoring report, Aug. 21, 1943, USSR 6900, G-2; press survey, "More Rumors on a Russo-German Peace," Oct. 8, 1943, *ibid*.; broadcast summary, Oct. 15, 1943, Germany 6900, *ibid*.

¹⁰⁵ Navy intelligence report, Oct. 15, 1943, Germany 6900, G-2. At the end of September a rumor circulated in Rome that a Soviet-German armistice would be signed on October 15. British Embassy, Bern, to British Foreign Office, Oct. 1, 1943, C 11735/55/18, FO 371/34438.

¹⁰⁶ Leland Harrison, Bern, to Secretary of State, Oct. 11, 1943, *FRUS*, 1943, 3: 708-09; press survey, Nov. 1943, USSR 6900, G-2. See also Bergen, Vatican, to German foreign office, May 4, 1943, AA, microfilm 39, frame 33750.

ported alleged discussions between German and Russian agents in Sofia.¹⁰⁷ No evidence has been found to substantiate any of these reports.

Paradoxically the peace scare reached its climax when the time for a Stalin-Hitler rapprochement had already passed. In August 1943, two months after the second Stockholm episode, Robert E. Sherwood reported that the atmosphere was "alarmingly reminiscent of that which preceded the Molotov-Ribbentrop pact of August 1939."¹⁰⁸ Although the Foreign Office in London received fewer reports about peace feelers in 1943 than they had in 1942, and although they discounted them as unfounded,¹⁰⁹ government officials in Washington were worried. They anxiously discussed memorandums such as "The Proper Course of Action for the United States in the Event Russia and Germany Effect a Compromise Peace."¹¹⁰ In October Roosevelt's chief adviser, Harry Hopkins, publicly admitted that possibility and proceeded to reassure the American people. He wrote in the *American Magazine*: "Russia, the keystone of the war, is still fighting grimly. If we lose her, I do not believe for a moment that we will lose the war, but I would change my prediction about the time of victory."¹¹¹

The presupposition that the Soviet Union had been so weakened by the war that it might seek peace because of the high price of victory was somewhat incongruously blended with the belief that her "postwar position in Europe will be a dominant one" and that "every effort must be made to obtain her friendship."¹¹² The two contradictory assumptions helped to shape the Western conviction about the necessity of concessions with respect to a postwar Russian sphere of influence in Europe. Such was the situation on the eve of the critical conferences scheduled to meet in late 1943: the conference at Moscow of foreign ministers in October and the meeting at Teheran of the Big Three a month later.

Their recent military successes notwithstanding, the Russians approached the Moscow conference from a position of weakness rather than of strength. They placed on the agenda only one item, and this item was suggestive of their uppermost priority: how to hasten the end of the war—by launching the Second Front and by other means.¹¹³ Regarding political questions, they had originally wanted the meeting to be "only of

¹⁰⁷ New York Times, Oct. 23, 1943; Laukhuff memorandum on conversation at the State Department with Father Odo, Oct. 19, 1943, 740.00119 EW/1934 DS; Wellington, memorandum on conversation at the State Department with Father Odo, Nov. 18, 1943, 740.0011 EW/32140, DS; report no. A-15077, Nov. 16, 1943, OSS.

¹⁰⁸ Robert E. Sherwood, *Roosevelt and Hopkins* (New York, 1948), 734.

¹⁰⁹ Woodward, *British Foreign Policy*, 2: 560.

¹¹⁰ Maurice Matloff, *Strategic Planning for Coalition Warfare, 1943-1944* (Washington, 1959), 286-87.

¹¹¹ Harry Hopkins, "We Can Win in 1945," *American Magazine*, Oct. 1943, p. 100.

¹¹² "Russia's Position," Aug. 2, 1943, *FRUS: The Conferences at Washington and Quebec, 1943* (Washington, 1970), 625.

¹¹³ "The Consideration of Measures to Shorten the Duration of the War against Hitlerite Germany and Her Allies in Europe," Oct. 19, 1943, *FRUS, 1943*, 1: General (Washington, 1963), 771-72.

preparatory character."¹¹⁴ But the Soviet delegation did not object to discussing such questions once they were introduced by the British and American representatives. The record of the proceedings shows that the Western delegates were at times more accommodating than the Russian delegates had apparently expected. For example, Molotov was surprised by Eden when he withdrew without debate his previous objections to the planned Soviet-Czechoslovak treaty, which would set a precedent for a client relationship between Moscow and the small nations of East Central Europe.¹¹⁵ Similarly Secretary of State Hull tried to meet the Russian concern for security with such radical proposals about the suppression of Germany that the foreign commissar was caught unprepared.¹¹⁶ The Western powers tried to convince Stalin that their friendship promised greater advantages than anything Berlin possibly had to offer. The Russian leader seemed convinced when, on the last day of the conference he walked up to Hull to deny "in the most sarcastic terms . . . reports . . . that the Soviet Union and Germany might agree on peace terms."¹¹⁷ And before the heads of state met at Teheran four weeks later Stalin made another important gesture to reassure his allies.

On November 12 Molotov handed to the United States ambassador, W. Averell Harriman, a memorandum that, along with the earlier hint by Vinogradov, is the only available document of Soviet origin that directly concerns the happenings in Stockholm.¹¹⁸ The foreign commissar stated that German agents had recently attempted to establish contact but had been immediately turned away. He mentioned Clauss and Kleist by name—an authoritative confirmation of their roles as intermediaries that bestows a measure of authenticity on Kleist's memoirs. The only discrepancy is in dates: Kleist, as well as Vinogradov, referred to early September, Molotov to mid-October. The different dating may have been necessitated by Kollontay's statement for *Daily Express* on October 3, in which she said, "There have never been any such feelers put by the Germans to my Legation."¹¹⁹ Too long an interval between the event and its disclosure could also have inspired undesirable queries about the motives for the delay.

As was obviously intended, the message promoted Western trust in the Soviet ally. Though belated and originally unexpected, this was in the last analysis Moscow's most important gain from its peace overtures earlier that year. Illustrative of the extent of the trust was the consent of the United States delegation at Teheran to being housed on the premises of the Soviet Embassy, which was undoubtedly well equipped with listening

¹¹⁴ Arkadi Sobolev to Eden, Sept. 29, 1943, annex C to memorandum no. 434, CAB 66/41.

¹¹⁵ Summary of proceedings, session of Oct. 24, 1943, *FRUS*, 1943, 1: 624-27.

¹¹⁶ Minutes of meeting, Oct. 25, 1943, *ibid.*, 632; see also 720-23.

¹¹⁷ Hull, memorandum, Oct. 30, 1943, *ibid.*, 687.

¹¹⁸ Molotov to Harriman, Nov. 12, 1943, *ibid.*, 502-03.

¹¹⁹ "Interview between Mr. Gordon Young and Mme Kollontay," Press Reading Bureau, Stockholm, to Political Intelligence Department, London, Oct. 7, 1943, N 5967/499/38, FO 371/36992.

devices.¹²⁰ Since there is no evidence that the guests took this special feature of their quarters into account, Stalin was thus given the unique opportunity to eavesdrop on their most intimate conversations.

At the Teheran conference Stalin appeared to be finally satisfied with his allies after they had demonstrated a willingness to meet him more than half way. They gave him a clear impression—clearer, again, than he had probably anticipated—that they would not obstruct his freedom of action in Eastern Europe. Concerning the all-important question of Poland, Roosevelt himself sought out the dictator to offer assurance that “personally he agreed with the views of Marshal Stalin” although Roosevelt “could not participate in any decision” because of concern for the Polish-American vote.¹²¹

The American and British statesmen hoped to win Russia's cooperation by satisfying what they perceived as its reasonable aspirations. But in the absence of clear understandings about what exactly was reasonable, their efforts tended to stimulate Russian aspirations rather than to restrain them. If earlier in 1943 the Soviet Union had signaled to the Germans that it would have been contented with return to the status quo as it had obtained in 1941, there were no longer any such indications at Teheran. On the contrary, Stalin hinted that his goals would expand with expanding opportunities: “There is no need to speak at the present time about any Soviet desires, but when the time comes we will speak.”¹²² At least one Western participant in the conference, Charles Bohlen, was convinced that those aspirations now extended even beyond the historic area of Russian interest. In what remains one of the best contemporary estimates of Stalin's intent, Bohlen concluded that the Soviet Union wanted to become “the only important military and political force on the continent of Europe” by reducing the rest of it “to military and political impotence.”¹²³

The roots of the developments that later culminated in the cold war should be sought in 1943 rather than in any other period in the history of the Great Alliance. With the cold war in mind, the significance of the separate peace prospects during that critical year is twofold. First, Stalin's inability to obtain a relatively modest territorial settlement from Germany led him to augment his goals while he continued fighting the war. Second, the British and Americans, disturbed about his leanings toward a negotiated peace, became inclined to tolerate more ambitious Soviet goals. Their attitude encouraged Stalin to test the limits of their tolerance later on. And it was his overestimation of those limits that eventually made conflict inevitable.

¹²⁰ The President's Log, *FRUS: The Conferences at Cairo and Teheran, 1943* (Washington, 1961), 461–64.

¹²¹ Minutes of Roosevelt-Stalin meeting, Dec. 1, 1943, *ibid.*, 594.

¹²² Minutes of meeting, Nov. 29, 1943, *ibid.*, 555.

¹²³ Bohlen, memorandum, Dec. 15, 1943, *ibid.*, 846.

A Soviet Historian Evaluates Stalin's Role in History

A Review Article by ROBERT M. SLUSSER

ROY A. MEDVEDEV, *K sudu istorii (genezis i posledstviia Stalinizma)*. Moscow: Samizdat. 1968. Pp. 1399. Photographically reproduced. New York: Alfred A. Knopf. 1972. \$20.00.

———. *Let History Judge: The Origins and Consequences of Stalinism*. Translated by COLLEEN TAYLOR. Edited by DAVID JORAVSKY and GEORGES HAUP. New York: Alfred A. Knopf. 1971. Pp. xxxiv, 566, xviii. \$12.50.

NIKITA KHRUSHCHEV earned his place in history twice: first, by his actions and policies as one of the most powerful Soviet political leaders over a period of many years; second, by the shock and stimulus he administered to Soviet historians with his attack on Stalin, launched in the secret speech at the Twentieth Congress of the Communist party of the Soviet Union (CPSU) in 1956 and deepened five years later at the party's Twenty-second Congress.

The complex response of Soviet historians and CPSU ideologists to Khrushchev's wide-ranging assault on Stalin's historical reputation has been analyzed by Nancy W. Heer in a work that takes the story down to mid-1967.¹ As of that date the great majority of Soviet historians held back from attempting a full-scale analysis of Stalin's career and crimes. When, in February 1966, twenty-five leading Soviet intellectuals, scientists, and artists addressed a letter to Leonid Brezhnev, first secretary of the CPSU, warning the party against rehabilitating Stalin at its then-imminent Twenty-third Congress, only two historians' names were included in the list of signatories.² Noting the absence of revisionist historians' names

¹ Nancy W. Heer, *Politics and History in the Soviet Union* (Cambridge, Mass., 1970). See the review by Warren B. Walsh, *AHR*, 77 (1972): 552-53.

² Peter Grose, "25 Soviet Intellectuals Oppose Any Elevation of Stalin's Status," *New York Times*, Mar. 21, 1966, p. 2. Grose mentions only eight of the signatories, among them Academician Ivan M. Maisky, who was not only a former diplomat (he had been Soviet ambassador to Great Britain from 1932 to 1943) but also a historian. See Maisky's contribution to a 1962 conference of Soviet historians, *Vsesoiuznoe soveshchanie o merakh uluchsheniia podgotovki nauchno-pedagogicheskikh kadrov po istoricheskim naukam 18-21 dekabria 1962 g.* (All-Union Meeting concerning Measures for Improvement of Training of Scientific-Pedagogical Cadres in Historical Sciences) (Moscow, 1964), 145-49, in which he calls for the publication of memoirs from the Stalin era, a genre to which he himself has made notable contributions. The other historian who

from the list of signatories, Dr. Heer comments, "No Soviet historian in any field, and certainly not a specialist in the CPSU, could acquire the kind of social distance from politics possible for a physicist or artist."³ A similar point is made in a recent review article in this journal. After summarizing evidence indicating the growing intellectual maturity and professional integrity of Soviet historians working in the pre-Soviet period of Russian history, Samuel H. Baron warns, "The constraints upon work in some of the more sensitive areas—such as the history of the Communist party of the Soviet Union, and contemporary history—have diminished little if at all."⁴

These observations help explain why the awesome task of writing a comprehensive historical analysis of Stalinism was finally undertaken neither by a professional historian nor by a party ideologist. Roy Medvedev, the man who successfully met Khrushchev's challenge, was trained in philosophy and made his career in the field of education.⁵ Drawn to the study of history by Khrushchev's revelations about Stalin's crimes at the Twentieth CPSU Congress and convinced by this bold action that the party had the inner moral strength needed to slough off the Stalinist legacy and return to Leninist principles, Medvedev joined the party and embarked on the study of its history under Stalin. The actual writing of the book, however, was undertaken only in 1961, following the Twenty-second CPSU Congress.⁶ The completion of the book in substantially the form we now have it came toward the end of 1968.⁷

During the long process of gestation there took place a creative interaction between the author and a small but keenly responsive circle of readers, friends, and critics, who analyzed and commented on the work in progress and in many cases provided valuable source materials for its enrichment. How could such an autonomous intellectual process, entirely lacking

signed the letter, not mentioned by Grose, was Academician S. D. Skazkin, one of the leading Soviet medievalists. See Skazkin's biography in *Sovetskaia istoricheskaia entsiklopediia* (Soviet Historical Encyclopedia), 12 (Moscow, 1969): 942.

³ Heer, *Politics and History*, 178. Dr. Heer's point is weakened but not entirely invalidated by the fact that two historians did sign the letter to Brezhnev; neither was, in her sense, a "revisionist" historian. Later in 1966 Skazkin was one of three historians who signed a letter to the RSFSR Supreme Soviet protesting the enactment of two articles in the criminal code (paragraphs 190-1 and 190-3) directed against internal political dissidents. The other historian signatories were V. M. Turok, a specialist in modern German and Austrian history, and P. I. Yakir, an intrepid critic of Stalinism and the son of a Red Army leader purged by Stalin. For a report of P. I. Yakir's arrest by the secret police, see the *New York Times*, June 22, 1972, p. 2. Copies of both the letter to Brezhnev and the letter to the RSFSR Supreme Soviet have been published by Radio Liberty, Munich, in its documentary series, numbers AS-159 and AS-273. I am grateful to Mr. Peter Dornan for help in locating these and other documents.

⁴ Samuel H. Baron, "The Transition from Feudalism to Capitalism in Russia: A Major Soviet Historical Controversy," *AHR*, 77 (1972): 728.

⁵ Biographical information on Medvedev is given in Professor David Joravsky's introduction, *Let History Judge*, x.

⁶ See Medvedev's foreword, *ibid.*, xxv. At the end of the work, however, Medvedev dates the beginning of the writing in August 1961 (p. 566). The Twenty-second CPSU Congress met in October 1961.

⁷ The terminal date for composition is given as August 1968 (*ibid.*), but some materials were added as late as January 1969, e.g., a reference on page 455 to a review of wartime memoirs in *Kommunist* (1969), no. 2, p. 127.

official sanction, take place in the Soviet Union? The answer lies in the fascinating phenomenon of "Samizdat," a term coined by Soviet intellectuals on the analogy of Gosizdat (State Publishing House), to designate the "self-publication" of manuscripts that for one reason or another have not found, or are not likely to find, a regular publisher. Typed by willing volunteers, who thereby assume a direct personal risk, Samizdat manuscripts circulate on a restricted scale, known to the secret police but somehow managing to elude their controls, and providing one of the principal bases for the development of the newly self-aware, critical intelligentsia in present-day Soviet society. Medvedev intended from the outset, however, that his book should eventually be published legally in the Soviet Union, for in his view only when the party had squarely faced the historical truth about Stalin and Stalinism would it be morally strong enough to overcome the baneful consequences of his rule. Medvedev therefore submitted the manuscript on completion to the CPSU Central Committee for approval. In an action carrying grave moral implications, the party rejected Medvedev's manuscript, thereby indicating its intention of whitewashing Stalin and closing the door to unbiased historical study of his career.⁸

It was at just about this time that scholars in the West first learned of the existence of Medvedev's work through a brief but laudatory reference to it in another Samizdat document, a "Memorandum" by the Soviet physicist Andrei Sakharov, which the *New York Times* published in translation on July 22, 1968.⁹ It is possible that Sakharov's well-intentioned reference to the book and the prominence given to it by Western news and propaganda media contributed to the decision by party authorities not to publish it in the Soviet Union.¹⁰ It seems highly probable, however, that the decision by the CPSU leadership to halt the movement toward unbiased historical study of Stalin's career had been taken before Sakharov's "Memorandum" was published in the West, and in fact well before Medvedev completed *Let History Judge*.¹¹

In early 1969, at a time when Medvedev was putting the finishing

⁸ For an official history of the CPSU that deals with Stalin in the spirit approved by party ideologists, see *Istoriia Kommunisticheskoi Partii Sovetskogo Soiuza v shesti tomakh* (History of the Communist Party of the Soviet Union in Six Volumes) (Moscow, 1964-), especially vol. 4, pt. 2 (1970), which achieves the dubious feat of covering the period from 1929 to 1937 with only the barest allusion to the Great Purge.

⁹ Andrei Sakharov's "Memorandum" is published in book form under the title *Progress, Co-existence and Intellectual Freedom* (New York, 1968). For the reference to Medvedev, see page 54. Sakharov describes the manuscript of Medvedev's book as being one-thousand pages long, a discrepancy that probably indicates he had seen only the first two parts; at the time Sakharov wrote the "Memorandum" Medvedev had not yet completed part 3, which begins on page 1031 of the Samizdat edition.

¹⁰ On the difficulties caused for Medvedev by the Western publication and broadcasts of Sakharov's "Memorandum," see the book by Medvedev's twin brother, the biologist Zhores A. Medvedev, *The Medvedev Papers* (London, 1971), 388-89.

¹¹ A pointer to the approximate time by which the decision had been taken is the publication by *Voprosy istorii* in February 1968 of an article dealing in a completely Stalinist spirit with one of the purge trials in the early 1930s. See D. L. Golinkov, "Razгром ochagov vnutrennei kontrrevoliutsii v Sovetskoi Rossii" (The Destruction of Hotbeds of Domestic Counterrevolution in Soviet Russia), *Voprosy istorii* (1968), no. 2, pp. 148-65. Golinkov's article was the third in a series that began in the issue of December 1967.

touches on his manuscript, the authoritative CPSU theoretical journal *Kommunist* moved closer toward an open rehabilitation of Stalin with the publication of both a review of recent memoirs by Soviet army commanders, which gave a favorable evaluation of Stalin's role as a wartime leader,¹² and an article by five relatively obscure authors setting forth the principles to be observed in dealing with the history of the CPSU, in effect warning Soviet historians to cease and desist from any further analysis of the phenomenon of Stalinism.¹³ Stung into direct action, Medvedev fired off a long letter to the editors of *Kommunist* in which he sharply criticized them for ignoring not only historical evidence about Stalin's crimes and blunders but also the directives of the Twentieth and Twenty-second CPSU Congresses condemning Stalin's "cult of personality." Medvedev's letter was never published by *Kommunist*, but it did appear, without his authorization, in *Posev*, an emigré journal published in West Germany.¹⁴ Professor David Joravsky, in his introduction to *Let History Judge*, argues that it must have been the KGB that sent Medvedev's letter to *Posev*. Be that as it may, publication of the letter abroad was used as the pretext for expelling Medvedev from the party. Then, early in 1970 *Posev* published over Medvedev's name a rather crudely written article attacking the Soviet leadership as corrupt. The journal claimed that the article emanated from Samizdat sources.¹⁵ Medvedev, in a letter dated March 25, 1969, to the Soviet press agency Novosti and to other news agencies, denied both that he was the author of the article and that it had ever circulated in the Soviet Union, and he charged that its publication represented "an intentional defamation."¹⁶ Made acutely conscious by these developments of the danger that his long manuscript on the history of Stalinism might be published in an unauthorized and inaccurate edition in the West, and

¹² Ye. Boltin, "Volnuiushchie stranitsy letopisi Velikoi Otechestvennoi Voiny" (Stirring Pages in the Chronicle of the Great Fatherland War), *Kommunist* (1969), no. 2, pp. 119-28. For the evaluation of Stalin, see page 127.

¹³ V. Golikov, S. Murashov, I. Chkhikvishvili, N. Shatagin, and S. Shaumian, "Za leninskiu partiinost' v osveshchenii istorii KPSS" (For Leninist Party Spirit in the Treatment of the History of the CPSU), *ibid.*, no. 3, pp. 67-82. For a rebuke to Soviet historians who persist in attempting to study Stalin's role in history, see page 73.

¹⁴ Roy Medvedev, "Vozmozhno li segodnia reabilitirovat' Stalina? Otkrytoe pis'mo v zhurnal 'Kommunist'" (Is It Possible to Rehabilitate Stalin Today? An Open Letter to the Journal "Kommunist"), *Posev*, 25 (1969): no. 6, pp. 25-30; no. 7, pp. 25-34. The editors appended a note (no. 6, p. 25), explaining that the letter had circulated inside Russia in a Samizdat edition, a copy of which reached them. According to Peter Reddaway, ed., *Uncensored Russia* (London, 1971), 421, 484n., Medvedev's letter first appeared in the *Khronika tekushchikh sobytii* (Chronicle of Current Events), a Samizdat periodical.

¹⁵ R. Medvedev, "Pravda o sovremennosti" (The Truth about the Present Time), *Posev*, 26 (1970): no. 1, pp. 39-45.

¹⁶ See Theodore Shabad, "Russian Historian Disavows Anti-Soviet Article Attributed to Him in West," *New York Times*, Apr. 26, 1970, p. 29. Shabad quotes from Medvedev's letter but does not give its full text. The letter in which a *Posev* editor attempts to justify the publication of documents from unidentified sources in Russia, which Joravsky cites in his introduction (*Let History Judge*, xi), was written in response not to Medvedev's letter to Novosti but to a manifesto published in *Le Monde* (Apr. 11, 12-13, 1970) in which Medvedev, Sakharov, and V. F. Turchin asserted that a "Letter to Brezhnev," which *Posev* had published over their signature, was a forgery. See "Les Savants Soviétiques et le 'Samizdat,'" *Le Monde*, May 6, 1970, p. 6.

blocked from publishing it in his own country, Medvedev thereupon took the weighty and responsible decision to authorize its publication abroad, but under conditions designed to ensure that it would be presented with scrupulous accuracy and in suitable form.

This prepublication background helps explain the character of *Let History Judge*, its author's intentions in writing it, and the form in which it is presented. Knowing that background one can better understand the moral stance of the author—that of a convinced Communist and a staunch admirer of Lenin—and its pervasive influence in the book. For it is Medvedev's underlying thesis that Stalinism was not the inevitable outcome of the political system established in Russia by Lenin and his colleagues but a monstrous distortion and perversion due in large part, though not exclusively, to fatal defects in Stalin's character. Because this thesis runs directly counter to the view, widely accepted in the West, that Stalinism was the logical consequence of Lenin's actions and policies, a number of reviewers have criticized Medvedev for what they regard as his failure to understand that link.¹⁷

This is not the place for an extended discussion of the problem, but two points should be made. First, Medvedev shows himself to be well aware of the roots of Stalinism in the policies of the early years of the Soviet regime, and although he does tend to exculpate Lenin from direct responsibility for Stalinism, he nevertheless gives the fullest and most searching analysis of this problem yet provided by any scholar. Second, acceptance of the thesis that Lenin's actions and policies led directly to those of Stalin has caused many scholars in the West to assume that the problem of the historical roots of Stalinism has been solved and no longer requires serious analysis. It is greatly to be hoped that Medvedev's treatment of the problem in *Let History Judge* will lead to a profound rethinking of it by Western historians and political scientists.

A closely related criticism of Medvedev's book has been that its importance lies mainly in the evidence it provides for tendencies within the present-day movement of dissident intellectuals in the Soviet Union, with the implication—sometimes made explicit—that there is little really new in what Medvedev has to tell us about the history of the Stalin era.¹⁸ There can be no doubt, of course, that *Let History Judge* does have value for an understanding of its author's political views, but its value as a contribution to history is far greater. In the words of Edward Crankshaw, it is "the first sustained, comprehensive, closely argued critique of the genesis, development and triumph of Stalin ever to be written from the inside."¹⁹

Accepting that judgment, I shall devote the remainder of this review

¹⁷ Reviews stressing this approach include those by Merle Fainsod, *Book Week*, Jan. 2, 1972, p. 4, and Leonard Schapiro, *Sunday Times* (London), Mar. 26, 1972, p. 40. For a balanced discussion of the problem, see Joravsky's introduction, *Let History Judge*, xii-xvi.

¹⁸ See, for example, the reviews by I. F. Stone, *New York Review of Books*, Feb. 24, 1972, pp. 14-22; Robert H. McNeal, *Russian Review*, 31 (1972): 179-81; and Robert Conquest, *Commentary*, 53 (1972): no. 6, pp. 80-89.

¹⁹ Edward Crankshaw in the *Observer* (London), Mar. 26, 1972, p. 36.

article to consideration of four substantive aspects of the book: the sources on which it is based; the contributions it makes to historical knowledge; certain weaknesses in the author's analysis of Stalinism due to his ignorance or neglect of non-Soviet studies; and the relationship between the original text and the edited translation.

THE RICHNESS AND DENSITY of Medvedev's book are the result in part of the author's ability to draw on an extensive body of memoirs and other materials dealing with Stalin and the Stalin era. Some of these have circulated in Samizdat editions.²⁰ Others have been published in the Soviet Union, for example, an account of imprisonment and torture during the Great Purge by the secretary of the party leader, Ya. M. Sverdlov.²¹ Works of fiction, or historical memoirs thinly disguised as fiction, have provided valuable insights—for example, a series of stories about concentration-camp life by V. T. Shalamov.²² Survivors of the purge trials of the 1930s have added their testimony; among the most important is a detailed account by M. P. Yakubovich, one of the defendants in the show trial of the so-called All-Union Bureau in 1931, which explains how the secret police worked over the defendants to prepare them for their roles in the trial.²³ Memoirs by Old Chekists (secret police agents whose service dates back to the early years of Soviet power) constitute a unique group of sources.²⁴

Many of the sources used by Medvedev have been published with official approval, for example, a number of histories of Communist party organizations in the Union Republics (Medvedev uses, among others, party histories dealing with Georgia, Armenia, Azerbaijan, Kazakhstan, Kirghizia, Uzbekistan, Tadzhikistan, Estonia, Moldavia, and the Ukraine).²⁵ Published after the Twenty-second CPSU Congress and, in most instances, before the official turn toward a partial rehabilitation of Stalin, these works provide much new information on the purge in minor-

²⁰ The most extensive collection of Samizdat materials in the West is that compiled by Radio Liberty in its *Arkhiv Samizdata*, sets of which have been deposited at the Center for Slavic and East European Studies at Ohio State University, the Center for International Studies at the Massachusetts Institute of Technology, and the Library of Congress. A number of the Samizdat documents used by Medvedev, which Professor Joravsky was unable to locate, are available in the Radio Liberty archive. If a revised edition of *Let History Judge* is prepared, it would be desirable to provide full data on all known Samizdat sources used by the author.

²¹ E. Ya. Drabkina, "Zimnii pereval" (Winter Solstice), *Novyi mir*, 44 (1968): no. 10, pp. 3-93.

²² For a listing of V. T. Shalamov's stories, see Reddaway, *Uncensored Russia*, 476.

²³ For a biographical sketch of Yakubovich, one of the outstanding figures in the dissident movement in the USSR, see *Khronika tekushchikh sobytii*, no. 10 (Oct. 31, 1969), as translated by Peter Reddaway in *Problems of Communism*, Mar.-Apr. 1970, pp. 51-52, and in *Uncensored Russia*, 397-402. The article by Golinkov, "Razgrom ochagov vnutrennei kontrrevoliutsii v Sovetskoi Rossii," reads like an attempt to discredit Yakubovich's far more accurate account of the 1931 trial, which circulated in a Samizdat edition.

²⁴ For example, the memoirs of S. O. Gazarian, which were circulated in a Samizdat edition, and the book by F. T. Fomin, *Zapiski starogo Chekista* (Memoirs of an Old Chekist) (Moscow, 1962).

²⁵ See the works cited on pages 203-07 and 221 of *Let History Judge*.

ity regions of the USSR. And professional historians have contributed their share. Medvedev quotes with telling effect speeches made at a conference of historians held in Moscow in December 1962—at a time, that is, when the impetus given by Khrushchev to the objective historical study of the Stalin era was still strong.²⁶

To call attention to all the new information provided by Medvedev would be a task requiring far more space than is available. Step by step, as he follows Stalin's rise to power, Medvedev adds previously unknown details or puts familiar facts in a new and more revealing light. When he reaches the Great Purge, his contributions become so numerous and cumulatively weighty that a dialectical transformation of quantity into quality takes place, and a more profound concept of the purge emerges, that of an organic process embracing every aspect of Soviet society. A particularly valuable cluster of new information surrounds the assassination of S. M. Kirov, with telling details contributed by friends, associates, and contemporaries of the fallen party leader. In this and similar sections *Let History Judge* creates the impression of a great collective outpouring of long-repressed memories. Sometimes Medvedev casually throws in a detail that illuminates an area previously shrouded in darkness. For example, he reports that in 1948, following the conference on biology at which T. D. Lysenko triumphantly revealed the party's endorsement of his theories, Stalin ordered A. A. Zhdanov into retirement, "using as an excuse Zhdanov's supposedly incorrect position" on doctrines espoused by Lysenko, "whom Zhdanov proposed to remove from the presidency of the Agricultural Academy."²⁷ This information (for which, unfortunately, Medvedev gives no source), together with data supplied by his brother Zhores to the effect that it was Stalin personally who approved Lysenko's report to the conference,²⁸ provides for the first time a valid basis for defining the positions of Stalin and Zhdanov in the biological controversy.

More important than any single new fact or group of facts, however, is Medvedev's profound and all-embracing grasp of the purge as a historical phenomenon. Particularly noteworthy are those sections of the book in which he analyzes the support rendered the Stalin cult by various groups in Soviet society.²⁹ Like the memoirs of Nadezhda Mandelshtam,³⁰ these sections of *Let History Judge* open up for historical study a wide panorama of changes in Soviet society during the Stalin era.

Medvedev takes a highly negative attitude toward non-Soviet studies of Stalin and the history of the CPSU. This attitude, needless to say, is as

²⁶ The speeches are included in *Vsesoiuznoe soveshchanie*. On the setting of the conference and its proceedings, see Heer, *History and Politics*, 145-63.

²⁷ *Let History Judge*, 484.

²⁸ Zhores A. Medvedev, *The Rise and Fall of T. D. Lysenko*, tr. I. Michael Lerner (New York, 1969), 116-17.

²⁹ See especially chapter 11, "The Conditions Facilitating Stalin's Usurpation of Power."

³⁰ Nadezhda Mandelshtam, *Hope Against Hope*, tr. Max Hayward (New York, 1970).

tendentious and one-sided as that of Western reviewers who find nothing new in his book. There are some areas in which non-Soviet historical scholarship has made contributions of fundamental importance to the study of the Stalin era, and by ignoring this work (or having been denied access to it) Medvedev has weakened his book. In particular his attempts to understand Stalin's motives for launching the Great Purge suffer from ignorance of some key links. Medvedev has apparently never seen, for example, the text of L. B. Kamenev's notes on his conversation with N. I. Bukharin in July 1928, a document that casts a revealing light on the struggle between Stalin and his rivals and on Stalin's character and strategy.³¹ Similarly, Medvedev has not made adequate use of *The Letter of an Old Bolshevik*, and has thus missed its penetrating analysis of high-level political maneuverings in the period preceding the assassination of Kirov.³² In addition Medvedev has failed to grasp the central importance of G. K. Ordzhonikidze's role in blocking Stalin's attempt to purge the party, as well as the significance of the link between Ordzhonikidze and Bukharin. Medvedev makes the assumption—plausible but erroneous—that G. G. Yagoda was a faithful executant of Stalin's orders throughout his career, thereby missing the significance of the telegram from Stalin and Zhdanov in September 1936 demanding Yagoda's ouster as chief of the NKVD.

It is a mark of Medvedev's stature as a historian, however, that he continued to deepen his understanding of Stalinism even while he was engaged in writing his book. One of the great values of the Samizdat manuscript of *Let History Judge* is that it enables us to follow this process in action. For example, it can be shown that Medvedev's attitude toward the problem of internal opposition to Stalin after 1929 underwent a significant change while the book was being written. The fullest treatment in the book of the case of M. N. Riutin in 1932, a turning point in the history of the party, is clearly a late insertion into the text (translation, pp. 142–43; Samizdat edition, p. 317a), which stands in striking contrast with references to Riutin elsewhere that treat the case as an unimportant

³¹ Kamenev's notes on his conversation with Bukharin first appeared in an illegal underground leaflet in the USSR entitled, "K partiinym konferentsiiam: Partiiu s zaviazannymi glazami vedut k novoi katastrofe" (Toward the Party Conferences: They Are Leading the Party Blindfold toward a New Catastrophe), signed "Bol'sheviki-lenintsy (Oppozitsiia)" (Bolsheviks-Leninists [Opposition]). *Die Volkswille*, a German Trotskyite journal, published a translation into German, from which it was retranslated into Russian and published in the Menshevik *Sotsialisticheskii Vestnik*, 9 (1929): no. 6 [196], pp. 10–11, under the title "Bol'sheviki o samikh sebe" (The Bolsheviks in Their Own Words). A few months later, having meanwhile obtained a copy of the original leaflet, the *Sotsialisticheskii Vestnik* added a brief additional section under the heading, "Dopolneniia k rasskazu Bukharina: Noch' 11–12 iulia" (Addition to Bukharin's Story: The Night of July 11–12), *ibid.*, no. 9 [199], p. 10. There is a copy of the Russian text in the Trotsky Archive at Harvard. No complete translation into English has been published.

³² *The Letter of an Old Bolshevik* was first published in the *Sotsialisticheskii Vestnik*, 16 (1936): nos. 23–24 [379–80], pp. 20–23, and 17 (1937): nos. 1–2 [381–82], pp. 17–24, under the title "Kak podgotovlialia moskovskii protsess (Iz pis'ma starogo bol'shevika)" (How the Moscow Trial Was Prepared [From the Letter of an Old Bolshevik]). An anonymous translation (New York, 1937) has been reprinted in Boris I. Nicolaevsky, *Power and the Soviet Elite* (New York, 1965), 26–65. The *Letter* is discussed in a Samizdat publication, the *Politicheskii Dnevnik*, no. 25, Oct. 1966.

and factually dubious incident (e.g. translation, p. 155; Samizdat edition, p. 344).

In regard to Soviet policy in Eastern Europe after World War II, the manuscript discloses a similar evolution in Medvedev's understanding. As originally written, the section dealing with this subject differed little from orthodox Soviet works that praise the "democratic socialist revolution" wrought in East European countries with Soviet aid.³³ After this section was written, however, the Dubček regime in Czechoslovakia reopened the subject of the purge trials of the early 1950s, and Medvedev inserted a long section (translation, pp. 476–77; Samizdat edition, pp. 1150–1150b)—based on articles by Eugen Loeb, one of the trial defendants, that were published in Prague in the spring of 1968—that differs sharply from the passage written earlier.

The moral to be drawn from these and similar instances is the urgent need for international cooperation among historians in studying the Stalin era. Tackling that central problem in modern Russian history is a task in which neither Soviet nor Western historians are likely to make substantial progress without the others' help.

Medvedev's book deserves and demands the most rigorous, scrupulous translating, editing, and publication. Few books in the field of Soviet history have been better or more conscientiously dealt with in this respect. The translator, the editors (especially Professor Joravsky, who assumed responsibility for the final editorial supervision), and the publisher have all met high standards, and the reader of the English-language version can rest assured that it is a substantially faithful rendering of the original text. Substantially, but not completely; and since we are dealing with a work of fundamental importance, it is necessary to note certain not entirely inconsequential differences between text and translation.

The number of actual errors is gratifyingly small. One may be mentioned for the record: a passage on page 110 of the translation reads, "And such a scapegoat was found: the specialists, the intelligentsia, *who had been tainted* before the Revolution" (my italics). What Medvedev wrote, however, was "And in fact such a scapegoat was provided for Stalin by specialists from among the old Russian (and Ukrainian) intelligentsia, *which had taken shape* before the Revolution" (Samizdat edition, p. 259, my italics).

There is a slight but perceptible difference in tone between the original text and the translation. Readers of the translation will recognize Medvedev's position as a convinced Communist, but they are not likely to realize fully the pervasive influence of this attitude, since many passages, in being edited for conciseness, have lost some of their personal flavor. Compare, for example, "In 1924 . . . Stalin did not seem dangerous" (trans-

³³ *Let History Judge*, 484. The original (Samizdat edition, 1138–39) is both fuller and more orthodox in spirit than the translation.

lation, p. 28), with "In 1924 . . . Stalin did not seem dangerous *for the revolution*" (Samizdat edition, p. 74, my italics).

In general, the translator and editors have shown great skill in condensing Medvedev's sometimes prolix and repetitive text without sacrificing anything essential. At times, however, the trimming process has been unduly rigorous. Consider these contrasting passages: "Old Bolsheviks now recall that Stalin had the pseudonym Vasili in 1912" (translation, p. 319); "In this connection some Old Bolsheviks recall that in 1912 Stalin, on escaping to Petersburg from exile and lodging together with Aron Sol'ts in one of the rooms at T. A. Slovatska's, used the party pseudonym 'Vasili'" (Samizdat edition, p. 702). Or these: "P. P. Postyshev, demoted from the Politburo and the Ukrainian Central Committee to a provincial post, then arrested and shot" (translation, p. 192); "P. P. Postyshev, a popular party worker, who held the post of second secretary of the Central Committee of the Communist party of the Ukraine. (Approximately one year before his arrest Postyshev was removed from the Politburo and assigned as first secretary of the Kuibyshev *obkom*)" (Samizdat edition, pp. 428-29).

More serious are deletions that have removed material of substantive value. Here are some examples: (1) Entirely omitted is an impassioned outburst (Samizdat edition, p. 413; cf. translation, p. 186) on the moral duty of the Soviet historical profession to tell the full truth about the purges. (2) There seems to be no good reason for cutting out an account of the repression of M. Leiteisen, a pioneer in the study of interplanetary travel, together with that of the engineers working at a construction bureau engaged in building a flying-wing aircraft (Samizdat edition, p. 501a; cf. translation, p. 229). (3) A passage on the intervention of Stalin and the Comintern leadership into the internal political struggle in the Communist party of Czechoslovakia in the late twenties has been completely eliminated (Samizdat edition, p. 890; cf. translation, p. 387). (4) A long section has been omitted concerning a novel by H. G. Wells that depicts the moral degeneration of a revolutionary leader—clearly relevant to Stalin and so recognized by a Soviet literary scholar in a work published in 1963.³⁴

My intention in citing these passages is not to impugn the quality of the translation and editing, which, I wish to emphasize, meet a high standard. The purpose is rather to indicate that specialists will find close study of the original manuscript indispensable. All honor, then, to the publisher, who has gone to great trouble to make available as legible as possible a copy of the Samizdat text. His services, like those of the translator and editors, are on the same high level of intellectual responsibility and devotion to the unending quest for historical truth as that of the book itself and its author.

³⁴ Samizdat edition, 744-47. See Ya. Kagarlitskii, *Gerbert Uells: Ocherk zhizni i tvorchestva* (Herbert Wells: An Outline of His Life and Work) (Moscow, 1963), 263-65.

Reviews of Books

GENERAL

ISAAC DEUTSCHER. *Marxism in Our Time*. Edited by TAMARA DEUTSCHER. Berkeley: Ramparts Press. 1971. Pp. 312. \$5.95.

GEORGE LICHTHEIM. *George Lukács*. (Modern Masters. New York: Viking Press. 1970. Pp. xi, 146. \$5.75.

ROBERT C. TUCKER. *The Marxian Revolutionary Idea*. (Publication of the Center of International Studies, Princeton University.) New York: W. W. Norton and Company. 1969. Pp. xi, 240. \$5.95.

It is hard to say anything new about Marxism, particularly if one is trying to defend some aspect of it. Marxism has been expounded, attacked, defended, and dissected in such a perennial world-wide torrent of literature that one might well wish for a moratorium on the subject.

This is not to denigrate the importance of Marxism as a historical phenomenon of the last hundred years and more. Marxism in one reading or another has become the frame of reference of most of the labor movement and intelligentsia over a large part of the globe, not to mention its privileged position as the official philosophy in Communist countries. But critical study of Marxism has made it fairly clear that the great success of Marxism is not based so much on its logical coherence or its self-proclaimed scientific method as on an appeal that is emotional, moral, quasi religious in fact.

What emerges most clearly from the three works under review, all aiming one way or another at a positive evaluation of the doctrine, is the essentially theological character of Marxism. Though deceptively couched in the language of science, Marxism rests ultimately on an unsubstantiated assertion of faith, above all

the faith that history will bring about the ultimate utopian society. Recognition of this fideistic character of Marxism helps provide a basis for understanding the development of official Soviet thought as a quasi-religious parody of scientific rationalism. Stalin did not have quite as far to go in "debasing" the original coin of Marxist doctrine as Mr. Deutscher maintains.

Flourishing as a faith movement in a supposedly scientific age, the vast influence of Marxism is all the more surprising on this account. One may fall back on the proposition that theological modes of thought—drawing from the need to believe—are imbedded in human nature and will attach themselves to whatever system of ideas happens to be popular. More specifically this trait of ideological theologizing seems to be rooted in the Continental European intellectual tradition, which apparently cannot dispense with grand system and hence remains vulnerable to the philosophical pretensions of Marxism. Two of the works under review proceed from this frame of reference: Deutscher readily admits to it, and Lichtheim deals tolerantly with Lukács's doctrinal blinders for the same reason. As for Tucker's attempt to appreciate Marx from the Anglo-American empirical standpoint, it points up another major component of Marxism's influence, equally illogical, as the doctrine of modernizing revolutionaries who hasten to "deradicalize" their faith after they take power.

Marxism in Our Time is a collection of Deutscher's lectures and essays, mainly of the mid-1960s, selected and edited by his wife after his untimely death in 1967. In subject they range from the methodology of *Das Kapital* and the history of Polish communism to the bureaucratization of the Russian Revolution

and the rejection of Marxism by the New Left. They reveal Deutscher as beyond a doubt the most creative and articulate Marxist thinker to appear anywhere since his mentor Trotsky—but a committed Marxist, be it understood, with no intention of questioning the basic premises of his faith. Deutscher's objectivity only went as far as the condemnation of Stalinism as a perversion of the true faith.

Not as much can be said for George Lukács, the Hungarian Marxist philosopher and literary critic, who as the giant of Communist Marxism, relatively speaking, only throws into relief the short intellectual stature of the other theoreticians of communism. In *George Lukács*, a brief intellectual biography in the Modern Masters series, the eminent historian of Marxism and socialism George Lichtheim does his best to convey to the English-speaking reader an appreciation of Lukács's critical intelligence and his perseverance under adversity. In chapters pursuing the successive directions of emphasis in Lukács's work—literary, historical, esthetic, philosophical—Lichtheim details his long but losing struggle to maintain his intellectual individuality within the shrinking confines of his Stalinist loyalty. Tragically, Lukács's attachment to the Germanic world of reified abstractions and his fideistic commitment to Leninist Marxism sapped his resistance to the Stalinist manipulation of the doctrine (so effectively condemned by Deutscher). Except for the brief moment of revolt in Hungary in 1956, Lukács fettered himself so well that the Soviet authorities hardly needed to pull the reins on him.

Against the convoluted efforts of the two European thinkers to justify their Marxist commitments, the essays by Robert Tucker in *The Marxian Revolutionary Idea* convey a breeze of cool realism. In these largely reprinted articles Tucker presents an articulate but familiar review of the basic principles of Marxism and endeavors to sift out its relevance for the Anglo-American mind of today. The haul is not very great—mainly the notion of rehumanizing man by transcending the state and the division of labor—while the major political successes of Marxism Tucker recognizes to have been accomplished under conditions of backwardness contradicting the theory and vitiating the ideal. One wishes that a few social philosophers

would forget Marx and start afresh to tackle the problem of the actual and the ideal.

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KENNETH E. BOULDING. *A Primer on Social Dynamics: History as Dialectics and Development*. New York: Free Press. 1970. Pp. viii, 153. Cloth \$5.95, paper \$2.45.

LESLIE SKLAIR. *The Sociology of Progress*. (International Library of Sociology and Social Reconstruction.) [London: Routledge and Kegan Paul;] distrib. by Humanities Press, New York. 1970. Pp. xvi, 272. \$10.00.

Kenneth Boulding's tract is an attack on all dialectical philosophy. In the flux of history and social dynamics he distinguishes two kinds of processes: dialectical ones centered on conflict and nondialectical, developmental, and evolutionary ones in which conflict is merely incidental. He contends that the latter has been by far the most important. In his comments on this theme, apart from saying much that is obvious to historians, he triumphantly demolishes the major tenets of Marxism. He also undertakes a cost-accounting of revolution, showing that evolution is cheaper (why did revolutions ever happen?). In conclusion he sketches a vision of a new universal humanistic history composed by man-computer teams. The appendix on Japan, "the first twenty-first century country," deals with the paradoxical appeal of Marxism in a country that "has one of the least dialectical of all histories." The tract is written in the social science jargon: "The development of democracy is the equivalent, in the sphere of social distance, to the reduction in the cost of transport and in the loss of power gradient in a geographical sphere" (p. 49); appropriately the author excels in reducing human phenomena to charts, graphs, and mathematical symbols. As for his command of historical reality one quotation suffices: "In the Soviet Union [Lenin's] war communism created a major famine and almost destroyed industrial production" (p. 93)—when in fact industry had come to a virtual standstill and famine had started even before the Bolsheviks took power.

Leslie Sklair, a sociologist at the London School of Economics, has written a more solid and ambitious work, though equally baffling to historians, defending the idea of progress as es-

sential for the theory and practice of sociology. He begins by tracing selectively the history of the idea of progress in the context of evolving sociological thought within the Western tradition in a manner familiar and useful to practitioners of the history of ideas. His historical survey ends with a discussion of the current distrust of "progress" in the writings of Jacques Ellul, Herbert Marcuse, and Ernst Gellner, "writers so divorced from life as it is daily lived that they do not recognize material and moral progress when they see it" (p. 107). He then sets out to "demonstrate that however ideologically unfashionable and scorned theoretically the idea of progress may be, it is essential to modern social thought, and that a sociological theory of progress is not only possible but necessary." The heart of his sociological theory of progress is "the sociological ethic." It establishes norms for deciding "whether an action contributes to the satisfaction of one or more of the individual or social needs." If it does the action is progressive. It would seem that in his learned sociological terminology the author merely re-establishes the traditional liberal social gospel: When there are problems to be solved in society, the best solutions—and the most progressive—are those that satisfy more people than any other suggested solution; they offer the greatest good to the greatest number.

The author claims to have made a special contribution to sociological theory by separating "innovational" progress from "non-innovational" progress. The former refers to the contributions made by science and technology and to the awareness (since the eighteenth century) of progress based on these achievements. As for the mind-boggling notion of noninnovational progress the author supplies too few facts for clear definition. He seems unaware that changes in attitudes, creeds, and institutions also constitute innovations. European development before 1700 certainly abounds with innovations of all kinds contributing to the rising confidence expressed in the notion of progress. What makes the author's argument even more obscure is his distinction between innovativeness and inventiveness; he argues, for instance, that Meiji Japan was high in terms of innovativeness and low in inventiveness (as are most contemporary developing countries). At the end he contends that innovational and nonin-

novational progress can be equally progressive on the road to modernity (whatever that undefined term stands for), which is his way of admitting that under present conditions "scientific" progress may even be regressive. What then constitutes truly progressive progress in our present world this eminently Europe-centered book does not reveal.

Reclining in his armchair after reviewing these books, a historian—any historian, I dare say—will exclaim: if only these authors knew more history! To judge by these works history is not dying out among the social scientists, as has been predicted. But, by Clio, what starved and ill-shapen history sometimes emerges from their labors!

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BENJAMIN B. WOLMAN, editor. *The Psychoanalytic Interpretation of History*. Foreword by WILLIAM L. LANGER. New York: Basic Books. 1971. Pp. x, 240. \$8.95.

In the first of three biographical studies in this collection Gustav Bychowski relies upon Trotsky's biography to illuminate Stalin's childhood and makes use of the "primal horde" as a theoretical model or explanatory device. He adds little to Khrushchev's analysis in terms of the "cult of personality."

Peter Loewenberg, less casual in his use of sources and psychoanalytic concepts, has been at pains to show that the "personal fantasies" of the Zionist leader, Theodor Herzl, were a "prelude to action" (p. 150). Certain of Herzl's experiences, in large part known from his *Diaries* and other sources, easily inferred from his fiction, indicate that he was unable to maintain ordinary personal ties of love or friendship but was able to derive an equivalent satisfaction from, on the one hand, his vision of the New Jerusalem and, on the other, "the mass response to his public utterances" (p. 179). Here Loewenberg has given considerable substance to the elusive term "charisma." He does not take up this problem from the other side, the relation of follower to leader, but he shows that Herzl, in adapting his fantasies to reality, "first codified" "a new ethos of militant resistance," a new "ego ideal" or "model of behavior," based in part on "the myth of chiv-

alry" (pp. 169-70), in part on the internalized "values of the dominant majority" (p. 171). This "new ethos" belongs chiefly to a later period ("was adopted out of the holocaust" [p. 1969]) and was not necessarily characteristic of Herzl's mass following in his lifetime. The confusion on this point derives from the sequence in which the argument is presented, not from its substance. This small matter aside, Loewenberg's paper is a forceful demonstration of the value of a psychoanalytic approach to the study of history.

Robert Waite has tried to answer the question, "why did Hitler himself become an anti-Semite?" (p. 195). His evidence ranges from the general background—the atmosphere of Vienna, von List's anti-Semitic pamphlets—to the particular circumstances of Hitler's life. His "tentative hypothesis" is that Hitler, burdened by incestuous desires and inclined toward possibly indulging in a "monstrous sexual perversion," came to feel "a truly massive amount of self-hatred and self-loathing which he projected onto the Jews" (p. 207). The argument is plausible but too dependent on the category of an alleged personality type, the anti-Semite. This category, taken seriously, would undermine both historian and psychoanalyst. *Who* is the target of projected emotions, Jew or Jesuit, Free-Mason or bourgeois, is chiefly a historical question. There is certainly nothing in the "monstrous sexual perversion" that would determine a projection specifically onto the Jews. Waite adduces other evidence, chiefly in connection with the Jewish physician who attended Hitler's mother, but his question, "why did Hitler himself become an anti-Semite?" I think really begs the question.

Psychoanalytically informed biography is of limited use to the historian. As Professor Langer remarks in the foreword, "The great hope . . . must be for help in understanding the group or mass actions of the past" (p. viii). Unfortunately the four remaining papers in this collection offer no help at all.

Two contributors address themselves to theoretical and methodological problems, but to little effect. On theoretical matters Robert Waelder adds nothing to Freud, while Waelder's interpretations of historical events go little beyond casual observation. As for the strange series of minilectures offered by the

editor, Benjamin Wolman, there is little to be said. *Destrudo* rides again.

Ronald Grimsley summarizes the views of several French literary critics more or less indebted to Freud. Robert Lifton, who draws his most telling examples from imaginative literature, has something more original to offer. He is "convinced that a universally shared style of self-process is emerging" (p. 34), a process "characterized by an interminable series of experiments and explorations—some shallow, some profound—each of which may be readily abandoned in favor of still new psychological quests" (p. 37). This view is not so much argued as enlarged upon in provocative and entertaining fashion. Perhaps Lifton's shrewdest insight is that the "equation of nurturance with a threat to autonomy" is "a major theme of contemporary life" (p. 45). He intends his essay "to be no more than a preliminary statement of an idea" that he hopes "to pursue more thoroughly in the future" (p. 33). Perhaps something will come of it. Meanwhile the reader interested in history and psychoanalysis might see Loewenberg's paper recently published in the *AHR* (76 [1971]: 1457-1502), "The Psychohistorical Origins of the Nazi Youth Cohort." Do not skip the footnotes.

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PAUL K. CONKIN and ROLAND N. STROMBERG. *The Heritage and Challenge of History*. New York: Dodd, Mead and Company. 1971. Pp. ix, 279. \$4.95.

In the past historians whose creative energies had waned would occasionally pause to reflect on the history of their discipline, producing encyclopedic volumes that did little to enhance their scholarly reputations. Now much younger scholars at the height of their creative powers are expressing in print their thoughts concerning the nature of their chosen discipline, commenting on its noble heritage and uncertain future. Such a shift has probably occurred because, as Conkin and Stromberg assert in their preface, "historians today feel more disquietude than at any time in our recent past." Such a "crisis of confidence" results in, among other things, much self-analysis. It no longer suffices

to let old men (or philosophers) ponder the nature of history.

As the title indicates, this particular effort to still the disquiet within the profession consists of two quite distinct parts. The first segment, written by Stromberg, is essentially a brief history of history; the second, written by Conkin, is an analytical piece similar in character to much of the literature on history written by philosophers of science.

In part 1 historians such as Herodotus and Tacitus are dealt with in a page or less each, the history of history for the entire ancient world requiring only sixteen pages. Unless the reader can bring to the material a rather substantial understanding of history and ancient cultures, it is questionable what purpose these introductory chapters serve. Anyone interested in the contribution to historical consciousness of the early Hebrews and Christians, for instance, would do much better turning to the writings of John Marcus or Paul Tillich. The treatment of historical scholarship since Ranke is more satisfactory and leads the reader into the issues that now trouble the profession. Stromberg's attempt to point out why the times are now more favorable to historical as contrasted with social science approaches is not very convincing. While the critique of scientism has accelerated of late, there is little reason to think that traditional historical approaches will be viewed as appealing alternatives.

In part 2 Conkin defines history as true stories about the past. Without raising the old relativists' doubts about an incomplete story ever being a true story, one can ask whether it is any longer adequate to equate history with the construction of narratives. Much that historians now do can be considered storytelling only if one has a most fertile imagination. But the key to Conkin's theoretical position is his attempt to establish the unique qualities of historical thinking. It places him within the confines of straight-line history as contrasted with some adventure toward a radical redefinition of historical scholarship. The focus is on epistemological questions, on matters of perceiving and knowing human experience, and on the relevance of such questions for written history. In five short, very carefully written chapters Conkin confronts the central theoretical issues

associated with causation, generalization, objectivity, and the human use of history.

Many will finish this volume reassured that historians need only continue down those paths clearly laid out by their eminent predecessors; some will be less certain that this represents an adequate response to the current "crisis of confidence."

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GEORGES LEFEBVRE. *La naissance de l'historiographie moderne*. Preface by GUY P. PALMADE. (Nouvelle bibliothèque scientifique.) Paris: Flammarion, Éditeur. 1971. Pp. 348. 38 fr.

NORMAN F. CANTOR. *Perspectives on the European Past: Conversations with Historians*. New York: Macmillan Company. 1971. Pp. xix, 359, 362. Cloth \$11.95, paper, in two volumes, \$4.95 each.

L. P. CURTIS, JR., editor. *The Historian's Workshop: Original Essays by Sixteen Historians*. New York: Alfred A. Knopf. 1970. Pp. xxiv, 326. \$7.95.

There was a time when historians were reluctant to write about historiography—whether in the guise of history of history, the methods of history, or philosophy of history. To judge from the pages of our professional journals this time is now over; historians are in a retrospective, analytical, and speculative mood. That is not altogether a bad thing; surely an unexamined profession is not worth pursuing. But the reflective and analytical books and articles now proliferating cannot all be magisterial contributions to our understanding of our craft. Certainly none of the three books under review is likely to advance our self-knowledge a great deal, though one of them, L. P. Curtis's collection, contains much suggestive material.

The publication of Lefebvre's course on the rise of modern historiography is largely an act of piety. The book is the "mise au point" of some of Lefebvre's last lectures at the Sorbonne in 1945 and 1946. Since Lefebvre was a great historian this posthumous collection is welcome simply as a product of his pen, especially since his lectures as printed here retain the personal tone, the informative aside, and the occasional ripe aphorism that he generally banished from his more formal work. But as a

history of modern history it is a distinct disappointment: compelled to move after two introductory chapters from the Renaissance to our day in slightly more than three hundred pages, Lefebvre sounds summary, downright breathless. He treats—or rather dismisses—Jacob Burckhardt in thirteen lines; even Karl Marx, one of Lefebvre's favorites, must make do with four pages. Such a hasty overflight makes superficiality inescapable. Beyond this, for all his interest in sociological history, Lefebvre here seems distinctly old-fashioned. Perhaps if he had depended less on Eduard Fueter's antique and unreliable *Geschichte der neueren Historiographie* (the "classic" work that, the editor tells us, served as the basis of Lefebvre's lectures) he might have ventured more freely into new fields of interpretation. As it is, we have a useful and brief reference work with some interesting asides.

Norman Cantor's more ambitious compilation of interviews with distinguished ancient and European historians is neither brief nor particularly useful. The only value of this strange production—and it is doubtless considerable—emerges from the occasionally inspired responses to Cantor's jejune interrogatives. Thus, Leonard Krieger manages to say something new about the emergence of German nationalism, and A. William Salomone ventures some fascinating speculations about nineteenth-century European culture. There are nuggets elsewhere in this ponderous volume; since its list of thirty-two contributors reads like a *Who's Who* of historians—including such old masters as Ronald Syme, R. W. Southern, A. G. Dickens, Asa Briggs, and Gordon Craig—it could hardly be without them. But what purpose is this designed to serve? It cannot sustain comparison with its pendant, John A. Garraty's *Interpreting American History: Conversations with Historians* (1970). Garraty, after obviously doing much careful homework, asked probing questions about the state of the field, the direction of research, and the particular contribution of each respondent. The result was an informative, often highly interesting, set of conversations that stand as supplements to textbooks and specialized monographs. Cantor's compilation, on the other hand, is a rival to the textbook, rather than its companion. His brisk and per-

functory questions—"Did the rise of industry completely transform English society?"—generally elicit fragments of potted short histories on various areas of ancient and European history; we can get the same subjects treated in more lucid and orderly fashion—often by the very historians here interviewed—in other places. On occasion, indeed, some of Cantor's respondents make stabs at evaluating the historical literature, with some questionable results. Frank Manuel gives it as his opinion that "for a new student in the field" of the Enlightenment "I would say that Becker's brilliant essay is still the best single work from which to get a feel for the eighteenth century." Becker was a sensitive and literate historian, but it is precisely the "feel" of the eighteenth century that we fail to get from him—if by "feel" we mean a sense of what the age was actually about. "*The Heavenly City*," Manuel continues, "is a major essay: it is profoundly respected even by Europeans who have written longer works on the same subject. It has been attacked, of course, for its errors in detail, but Becker wouldn't have cared." I can only wonder at such peculiarly unprofessional idealization: the essay has been attacked for far more than "errors in detail," as Manuel must know; but even if it had been attacked only for that, Becker would have done well to care—does good history not depend on accurate detail? Of course, opinions like these are not Cantor's, the interviewer's, fault—the introduction and headnotes, on the other hand, are; apart from the factual information they contain, they are patronizing, tasteless, and embarrassing.

Curtis's collection of original essays is embarrassing as well, but on purpose. Curtis has had a new idea, which I can only applaud—to ask historians to talk freely about their "frame of reference" and their way of working that emerge in the writing of one of their books. Since historians' autobiographies are rare and on the whole unrevealing, the spectacle of a historian—or, as here, sixteen historians—in undress is bound to be a startling sight. While the essays here collected are of varying merit, the enterprise cannot help but be immensely useful; if it encourages other historians to bare their ways of thinking, their false starts, their passionate convictions and occasional disap-

pointments, the profession will be all the richer for it.

Since the middle of the nineteenth century, since William Whewell and Claude Bernard, students of scientific procedure have argued against the Baconian notion that we compile facts first and make our theories later; in recent years philosophers, historians of science, and distinguished practitioners, like Karl Popper, Thomas Kuhn, and P. B. Medawar, have strengthened our theoretical understanding and extended our factual knowledge of how natural scientists actually work; they have firmly discriminated between the logic of science (which tells us how a statement can be proved to be true) and the psychology of scientists (which tells us how they arrive at their statements in the first place). We urgently need a similar psychology of historians—perhaps more urgently than a psychology for history, a subject that threatens to engulf us these days. A psychology of procedure requires large amounts of factual information, and the present collection of partial autobiographies makes an admirable start. Autobiography is an art, and most of Curtis's collaborators—the fifteen of fifty-two who finally accepted the editor's invitation—are a little uncomfortable with it. Yet most of them manage to be informative, if not always wholly by intention. It would be invidious to single out any particular essay as "the best"; given the nature of the enterprise, each reader must choose the essay, or essays, he finds most instructive. My own favorites are Lynn White's witty and forthright account of his growing interest in the function of technology in civilization, John William Ward's economical and self-critical reappraisal of his study of Andrew Jackson as a culture hero, and the late Joseph Levenson's thoughtful explanation of his intentions in writing his great trilogy on Confucian China. Other contributors shed light on their work—R. R. Palmer on the *Age of the Democratic Revolution* (1959), John Pocock on his fascination with time, George Rudé on his long-term occupation with crowds. Sometimes the light is a little lurid: in the course of psychoanalyzing his psychoanalytical biography of Lou Salome and his subsequent work on Leopold III of Belgium, Rudolph Binion casually connects literature with life through the situation of "a young lady of my

close acquaintance" who "reenacted a traumatically unfulfilled love after five years of brooding over it." After she had gone "to pieces this second time round," Binion "psychoanalyzed her at her urgent request—and in the process solved, until further notice, my Lou-Leopold problem left outstanding." L. P. Curtis has opened Pandora's box; we may expect further revelations with trepidation but also with real interest.

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HELMUT HIRSCH. *Lehrer machen Geschichte: Das Institut für Erziehungswissenschaften und das Internationale Schulbuchinstitut. Ein Beitrag zur Kontinuitätsforschung.* (Schriftenreihe zur Geschichte und politischen Bildung.) Ratingen: A. Henn Verlag, 1971. Pp. 264.

In 1969 the American Historical Association cosponsored the publication of Donald W. Robinson's *As Others See Us: International Views of American History*, in which successive American history topics were treated by historians from different nations. The result was a startling lesson in relativism. There are apparently several versions of the history of the French and Indian Wars: the American, the English, and the French; the Indian version remains to be written. As the international community becomes more closely knit, attempts will have to be made to give history a global perspective.

Efforts to write history without national bias began after World War I. In 1925 the Scandinavian countries concluded the first agreement for revision of textbooks by mutual discussion. In 1933 the first international treaty agreed upon by Brazil and Argentina provided for periodical textbook revision. By 1938 twenty-two nations had some form of agreement. Germany and France reached one in 1935.

This book describes one significant German attempt to play its part in textbook revisions of the post-World War II era. The origin of the International School Book Institute located at Brunswick is to be found in the Research Institute for Education founded there in 1930 by August Riegel. Riegel was later dismissed from his post by the Nazis and the institute was put in abeyance. But the library of the foreign history texts accumulated by Riegel found its way

into the hands of George Eckhart and became the nucleus of the International School Book Institute he founded in 1949.

The purpose of the institute is quoted as being inspired by a Polish historian who crisply exhorted his colleagues "to try to eliminate all tendentious meanings from history." Actually the more immediate motivation was to redeem Germany from having been "diverted into narrow channels" by the Nazi interlude.

The book proceeds to describe in great detail the circumstances of the foundation and development of the institute. In furthering his work George Eckhart sought the support of several organizations both inside and outside his country. By 1965 the institute became affiliated with the Council of Europe. Its present name is Schulbuch Zentrum für Geschichte und Geographie der Länder des Europarats.

The functions of the institute consisted in organizing conferences, collecting library materials, rewriting textbooks, and making representations to other countries for rewriting textbooks. The book recounts instances of Belgian and English hostilities, the East-West controversies, and clashes with the Americans. In spite of such difficulties Eckhart, who, in addition to the directorship of the institute and a professorship at the Kant Hochschule at Brunswick, also served as member of the German Commission to UNESCO, was able through his contacts to resolve successfully mutual revision conflicts with countries such as the United States, France, Belgium, and Japan.

The book is unfortunately replete with irritating hints of the author's personal vanity as well as excessive local and institutional pride and German national patriotism. This mars not a little the fine story of a courageous and serious attempt to denationalize history.

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LOUIS GOTTSCHALK *et al.* *The Foundations of the Modern World.* (History of Mankind: Cultural and Scientific Development, Volume 4. Prepared under the auspices and with financial assistance of the United Nations Educational, Scientific and Cultural Organization.) New York: Harper and Row for the International Commission for a History of the Scientific and

Cultural Development of Mankind. 1969. Pp. xxxi, 1133. \$20.00.

The volume at hand is the fourth in UNESCO's attempt to examine mankind's cultural and scientific development as a single story. Quite apart from any objections to the implied distinction between science and culture, I feel compelled to register serious doubts about the entire enterprise as conceived and executed.

First, however, one should be clear about the conception itself. The preceding volumes deal, respectively, with "Prehistory and the Beginnings of Civilization," "The Ancient World," and "The World, A.D. 400 to A.D. 1300." The succeeding two volumes are entitled "The World in the Nineteenth Century" and, forthrightly, "The Twentieth Century" (when the definition of the world obviously became complicated).

Certain problems presented by this fourth volume will be readily apparent to most students of history. The author-editors, who generously acknowledge the critical collaboration of no fewer than sixty-two consultants, including seven who appear to have been most deeply involved, were doubtless justified in treating the period 1300-1775 as one having a genuine coherence. Most of us in the European field are uneasy when we are cut off too sternly from the Middle Ages. The record of an "old Europe" often challenges our standard periodization. The troublesome question remains whether the record of other civilizations helps very much in this regard.

What is still more troublesome, however, is the decision of Professor Gottschalk and his colleagues to adopt a principle of organization that seems to me not to work. After roughly 100 pages of political summary we confront over 350 pages of religious history, sometimes tintured with metaphysics. Then, in succession, come paired chapters on political and social thought, "literary communication and belles-lettres" (an uneasy distinction), and the visual arts and music. Finally, there are 250 pages on science, technology, and education.

Granted that every historian has to make choices all the time between topical and chronological approaches, the line followed in the volume under review is so defiant of history's wholeness as to cast doubt on the very idea of a

"history of mankind." To take one example, few students, reading early in the book about Jansenism as a theological position, will be able to remember its role (with Gallicanism) when they get back to seventeenth- and eighteenth-century political thought some hundreds of pages later.

The author-editors deserve credit for having faced up to this difficulty and even, in a prefatory "Note on the Preparation and Editing of Volume IV," having reprinted the objections of Professor Roland Mousnier. Those objections, which still appear to me valid, include the following: "The links between the facts studied in each case and those considered in other categories are rarely shown. . . . There is lacking a general view of all these human activities" (p. xviii). Professor Gottschalk answers this complaint quite reasonably by pointing out that every scheme of organization involves a trade off, a surrender of some advantages to secure others. On balance, however, it seems that Professor Mousnier is right in pressing his query: Is the only object of this exercise an encyclopedic work of random reference?

The dissatisfaction that remains with me, however, has a different source. It has to do with the very idea of "world history" dominated by the representational structure of the United Nations and written by negotiation, which has its place—elsewhere. Asian history, African history, and Latin American history all deserve to be studied rigorously in their own terms. Those terms need not, and should not, exclude careful comparisons with other fields.

What appears to me to happen, however, when sixty-five historians, including seven Russians, a Syrian, an Israeli, two Indians, five Hungarians, and two South Vietnamese, "collaborate" with dozens of other scholars from other lands is that history, as an assignment of recovery and interpretation, simply gets lost. One final illustration is in order. Perhaps, given limitations on space, British political development between 1300 and 1775 could not be assigned more than the two paragraphs it receives on page 12. But I have yet to be convinced that England in those centuries was less significant to the history of mankind, *all mankind*, than were Mandingo, the Mali Empire, Kano, and the Ashanti federation, dealt with earlier and at greater length (pp. 2-4).

The above expresses no racism, of any of the currently available varieties. It does express doubts about history as a product of bargaining. Soon after the Second World War some German and French historians got together, contritely, and agreed that Louis XIV was rapacious but a prince of his time. They also agreed that the Franco-Prussian War was both sides' fault. The propositions were doubtless true, but neither advanced historical studies very much. The global approach has, so far at least, offered us nothing better.

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Third International Conference of Economic History, Munich, 1965. Volume 2. (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Congrès et colloques, Number 10.) Paris: Mouton & Co. 1968. Pp. 304. 48 fr.

The twenty-seven papers and the ten discussions assembled in this volume represent the combined output of only one section (Agricultural Production and Productivity) of the Third International Conference of Economic History, which met in Munich in 1965. Despite the sometimes heroic efforts of the late Jean Meuvret (who was responsible for the section) to impart cohesion to such a large and disparate body of papers, it is patently impossible to seize upon a single theme other than a common interest in the measurement of agricultural production and some explanation of the vicissitudes of agricultural growth in various areas of the world. Needless to add, all areas are not treated in proportion to their importance to agricultural history. The European world clearly dominates (twenty-one of the twenty-seven papers); Japan is represented by two papers; the United States, Mexico, Argentina, and India by one each. Without treating an international conference of historians like the Washington Naval Conference and recognizing that availability of sources and research talent must play a determining role in the assignment of papers, greater effort should still be made to solicit research from economic historians of Asia, Africa, and the Americas at future international conferences, especially in this area of agricultural economics and rural sociology. And surely, to have only two short

papers on England and none on the Low Countries—the pioneers in agricultural change—is unfortunate in the extreme.

About half of the articles in the collection treat such problems as the completeness and authenticity of sources (the relative value of private account books, church *dîmes*, and public fiscal records), the arithmetic of crop yields (by seed, by area, by manpower), the relation of crop rotations to livestock, and the relation of area, soils, population density, and land tenures to mechanization and nonmechanical innovation. No doubt, as a result of the Munich conference we know more about how to measure productivity—at least of cereals. But with the exception of R. A. C. Parker's brief paper on the Coke estate in Norfolk, the papers treating Western Europe are not primarily concerned with landlord-tenant relations, reinvestment rates, or the broader social aspects of agricultural production. The representatives from Central and Eastern Europe, on the other hand, were less hesitant to enlarge the scope of their assignment, often tracing the agricultural history of entire countries over several centuries. Thus, V. K. Yatsounski of Moscow's Academy of Sciences presents a seventeen-page paper on Russian agricultural production from 1500 to 1917, while László Makkai of the Institute of Social Sciences in Budapest presents a nine-page résumé of Hungarian agriculture in the "era of late feudalism," that is, from 1550 to 1850. Contrast this perspective with Emmanuel Le Roy Ladurie's nine pages on "Grain Yields in Languedoc," replete with tables and tight arithmetic.

As examples of economic history pursued in Eastern Europe, consider the papers of Yatsounski and François Matejek on Russian and Czechoslovakian agriculture in the four centuries from 1550 to World War I. Their use of a threefold periodization of agrarian history—"feudal," "capitalist," and "socialist"—creates some initial difficulties, since it is not always clear whether political, ideological, or economic events determine the transition from one epoch to the next. In these two papers the emancipation of the serfs in 1848 and 1861 ushers in the *époque capitaliste* and initiates major changes in the rate of agricultural growth. Matejek establishes a veritable "agricultural revolution" in the thirty years after

1848, punctuated by the planting of new forage crops, the near elimination of the fallow, and the introduction of the potato and sugar beet along with expansion of such derivative Czech industries as brewing, distilling, and starch-making. Yatsounski traces a similar, if less dramatic development in Russian agriculture after 1861, but he attributes much of it to the initiative of the larger landlords rather than to Matejek's state agricultural schools, which Matejek claims spread technical know-how to the *peuple*. Yatsounski in fact believes that the agrarian policies of the tsarist government that aimed at increasing the number of small holdings actually "inhibited productive forces on the land." His colleague, A. M. Anfimow, makes the same observation about the Stolypin reforms of 1906-14. Matejek, on the other hand, not only refuses to condemn the small holder, but he even attributes substantial increases in bovine livestock in the 1870s to the *paysan* rather than to the large landlord. It would appear that Matejek shares some of Jean Meuvret's respect for "agrarian individualism"—even "petty bourgeois individualism"—as alternative promoters of agricultural growth.

Economic issues are not always kept distinct from a bias in favor of agrarian populism. All papers concerning Eastern Europe agree that serfdom was the great stumbling block to agricultural progress—so great in fact that it obscures other possible obstacles. Furthermore, I am not convinced that the system of *latifundia* per se (and one would like to know more about areas and management) blocked innovation on the land. Some attention should be given to the psychology of landlords. It may well be that the Polish magnates spent all of their profits from grain exports on luxury goods from the West, but, as the English example suggests, large holdings were not always starved for reinvestment capital. In one of the discussions Pierre Vilar suggested that there may have been an aristocratic *laissez-aller* psychology (as in Spain) that remained after the formal seigneurial system had gone but before the new "mentality of capitalist exploiter" (or entrepreneur?) had arrived. Perhaps there are landlords and there are landlords, regardless of the system of land tenure. But psychological aspects of the problem (despite the existence of volu-

minous private estate papers) were lightly treated, to say the least. Miklos Szuhay of the Karl Marx University in Budapest seemed to capture the main thrust of his own and his colleagues' interpretation when he concluded that "real" agricultural growth could only take place "by breaking up the system of large [private] landholdings . . . and, above all, by the socialist reorganization of agriculture." Does this mean that there are "efficient" and "inefficient" forms of large-scale agriculture? In any event, there is not much place here for the emancipated peasant, either as independent farmer or as promoter of agricultural growth.

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ROBERT SCHWOEBEL, editor. *Renaissance Men and Ideas*. New York: St. Martin's Press. 1971. Pp. xxii, 137. Cloth \$6.00, paper \$2.95.

This collection is designed "to bridge the ever-widening gap between the world of the specialized scholar and that of the undergraduate student, between the scholarly journal and the textbook." In his introduction Robert Schwoebel performs the impressive feat of touching on general problems raised by the concept of the Renaissance and bringing together the common themes of the nine articles that follow. Without talking down to the book's intended audience or making a cheap plug for the relevance of the Renaissance he indicates that thinkers of that period concerned themselves with perennially perplexing problems: "the nature and uses of power, the responsibility and possibility of education, the limits of reason, the sources of moral authority, the social and ethical implications of scientific discovery and technology." The serious but vigorous tone he sets is maintained throughout the volume; all the contributors write well and offer sound and illuminating interpretations of their material.

Renaissance Men and Ideas includes the following studies: "Renaissance Humanism: Petrarch and Valla" by Jerrold E. Seigel, "Printing and the Spread of Humanism in Germany: The Example of Albrecht von Eyb" by Rudolf Hirsch, "The English Renaissance: Sir Thomas More" by George Richard Potter, "Machiavelli" by Felix Gilbert, "Pius II and the Renaissance Papacy" by Robert Schwoebel, "Lu-

ther as Scholar and Thinker" by Lewis W. Spitz, "Copernicus and Renaissance Astronomy" by Edward Rosen, "The Paideia of a Renaissance Gentleman: Castiglione's *Book of the Courtier*" by Roslyn Brogue Henning, and "Montaigne on the Absurdity and Dignity of Man" by Donald M. Frame. All are of very high quality, and intelligent undergraduates should find each interesting and informative. I felt that two of the studies were especially helpful in putting more or less familiar material into a new and more viable pedagogical framework. Other essays in the volume will no doubt seem particularly enlightening to scholar-teachers with different backgrounds and interests.

Of all the contributions to a collection where the emphasis is on intellectual history Hirsch's study comes the closest to hinting at the insights that quantitative approaches can bring; it also helps to confirm Elizabeth Eisenstein's recent assertions. His thesis that Northern Europe was not as far behind Italy in concern for the classics as has usually been supposed is supported by impressive lists of German first editions of classical texts put out for the most part by printers who did not specialize exclusively in scholarly works. A consideration of the *oeuvre* of one early figure, Albrecht von Eyb (1420-75), suggests further that some modification of the traditional notion of a time lag in northern humanism needs to be made. Primarily a popularizer, Eyb in his *Margarita poetica* and *Ehebuch* wove together a tissue of classical citations and extracts designed for practical application. The fact that these books reached a very extensive audience (an estimated 4,000 copies of the *Margarita* and 7,000 of the *Ehebuch* were published) indicates that the new medium of printing provided some degree of exposure to humanist ideas for Germans not fortunate enough to travel to Italy or to participate in aristocratic learned sodalities.

In the study on Machiavelli we find a scholar's distillation of many years' research on a persistently puzzling subject. Not only does Felix Gilbert demonstrate complete mastery of the immense literature on Machiavelli; Gilbert provides a key to understanding why almost every intellectual generation has felt a need to reassess the meaning and intention of his works. In Gilbert's view (as in Isaiah Berlin's) modern scholarship can come closer than that of pre-

vious epochs to taking Machiavelli's stark world view at face value because the twentieth century, like the early sixteenth, is a time when old certainties no longer hold. The central consideration in Machiavelli's writings, Gilbert maintains, is power in the context of the ruler-subject relationship—power that, when effectively institutionalized, can prolong the life of a state at the apex of the inevitably recurring cycle of historical development. In Machiavelli's age "the feeling of having lost control over events was dominant": hence his obsession with the redefinition of the political world as a realm of flux and with the revaluation of "virtue" as the ability to control men and events. Gilbert's essay, along with the others in this volume, is a solid, provocative reinterpretation that has much to offer the specialist as well as the neophyte in Renaissance studies.

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J. G. ROWE and W. H. STOCKDALE, editors. *Florilegium Historiale: Essays Presented to Wallace K. Ferguson*. [Toronto:] University of Toronto Press, in association with the University of Western Ontario. 1971. Pp. xiii, 401. \$16.50.

Sixteen scholars have contributed to honor Wallace K. Ferguson on his seventieth birthday—fourteen (one of them the honoree's brother) with articles on different aspects of the Renaissance and two as careful, imaginative editors. The product of this energy is the sort of book I like: 246 pages of text and 117 pages of notes and apparatus, not counting illustrations. Furthermore, the notes are largely to primary materials, manuscripts, and old books; and the articles bear on Ferguson's seminal work on the Renaissance and the idea of the Renaissance, in and beyond historiography. This means that the contributors have consciously worked to clarify problems Ferguson raised in his books and articles: two (Paul Kristeller and Myron Gilmore) write on Erasmus; four on humanists (Hans Baron on Petrarch, Eugene F. Rice, Jr. on Lefèvre d'Étaples, Arthur Ferguson on early Tudor humanists, and Denys Hay on the geographical humanists who contributed to "the idea of Italy"); and two (Millard Meiss and Edward Lowinsky) on the arts. Frederic Lane and Felix Gilbert offer

precise delineation of problems in Venetian politics and historiography; Nicolai Rubinstein provides a quiet, brilliant analysis of Machiavelli's uses of the variously loaded word *stato*; J. R. Lander analyzes the relations of the first two Tudor monarchs to their troublesome (and essential) magnates; finally, J. R. Hale presents another folio on his perennial topic of Renaissance wars and rumors of wars, this time a study of the sermons dealing with warfare from the Armada to the Civil War.

The articles are very different from one another. Both Baron and Rice deal with what Renaissance scholars call "medievalism," Baron with Petrarch's Augustinianism, and Rice with Lefèvre's publications of medieval mystical texts, but Baron does not quite define the important revision of Petrarch's life that he offers, though Rice is clear about what Lefèvre's industry meant in French intellectual life. In another context Myron Gilmore presents an unquestionably disputatious Erasmus, never letting pass an opportunity in his own defense, who, at the article's end, folds his hands in the irenic gesture historians prefer to see him take. Arthur Ferguson's graceful article recapitulates much from his 1962 book but lays special stress on the English humanists' sense of history, here interpreted as man's emergence at the beginning of his development from a disagreeable state of nature, rather than as history in any modern sense.

Lane's and Gilbert's articles are models in their kinds; both show how and why things happened—why the Venetian council had to be enlarged and how that enlargement was effected; why Venetian historiography in fact took a radically new turn in the Renaissance, for reasons laid out by Mr. Lane. Meiss's article is an iconographical contribution that abides by its stated limitations; Lowinsky manages as ever to speculate beguilingly, this time via Anne Boleyn and her music man Mark Smeaton. Rubinstein's article is a model of intellectual analysis, giving far more than it promises; Lander's researches make even plainer than before the forcefulness of Henry VII in a very ticklish situation indeed. Marvin Becker's study of the "quest for identity" in the early Renaissance develops a theme on which I have earlier heard him lecture: despite citations to the right authors (Ladner, Leff,

Kantorowicz, Trinkaus, Garin), the use of "in" phrases (identity crisis, ego support, socially-defined roles, civic persona, scheme of ceremonial identity), and references to topics like love, chivalry, historiography, the Church, government, and commercial enterprises, I had been unable to understand his premises or the order of his argument when I heard them spoken. Now that I read this article (note that I am not criticizing what some historians may consider sociological jargon; that I think I can understand and certainly find useful), I find that my earlier deafness has become blindness. I don't know who the early Italian Renaissance was that it should seek its identity: one has to be careful with this sort of subject, when even Huizinga often nods. Becker has, however, tried to synthesize some of the many problems Ferguson has frequently referred to: his is the zeal of the house. For its many different virtues scholars will have to use this *Festschrift*; what is more telling, Wallace Ferguson will use it—and what nicer present could a man hope to receive on his birthday?

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ERNEST F. DIBBLE and EARLE W. NEWTON, editors. *Spain and Her Rivals on the Gulf Coast*. (Proceedings: Second Gulf Coast History and Humanities Conference.) Pensacola: State of Florida, Department of State; Historic Pensacola Preservation Board. 1971. Pp. vi, 143. Cloth \$5.00, paper \$3.00.

During the three centuries of history from 1513 to the early 1800s three European powers, Spain, France, and England, contended for supremacy in the territory from the Florida peninsula to Texas. In this struggle Spain appeared first and led the way in promoting exploration and colonization, but the rival nations subsequently entered the contest for primacy and eventually made their own significant contributions in the creation of the patterns of life so characteristic today of this part of the American scene.

At the Second Gulf Coast History and Humanities Conference, held in December 1970, the five formal papers read, which constitute principally the subject matter of the present publication, are all related to the colonial beginnings of this Gulf region and are also "in-

terdisciplinary," as a glance at their titles will reveal. They are as follows: "Gulf Coast History: An Overview" by Alfred Thomas; "French, Spanish, and English Indian Policy on the Gulf Coast, 1513-1763: A Comparison" by John J. TePaske; "Revolt in Louisiana: A Threat to Franco-Spanish Amistad" by J. Preston Moore; "The Spanish Gulf Coast Cultural Assemblage, 1500-1763" by Hale Smith; and "Gulf Coast Architecture" by Samuel Wilson, Jr. Also included is an appendix prepared by William S. Coker and Jack D. L. Holmes dealing with the sources for the history of the Spanish borderlands.

The reader of these scholarly studies is likely to increase his understanding and appreciation of the Gulf Coast and its history. Perhaps he may also come to recognize with Dr. Thomas (and as did Herbert Bolton and others, including myself years ago) the grave error of the traditional school of historians who view the origins of American civilization "almost exclusively in terms of 13 English colonies along the Atlantic Seaboard." How ironical this position is when, in fact, "the vast region from Florida to California" had its own earlier and separate colonial beginnings! It is well to add that this publication is greatly enhanced by the inclusion, in connection with Wilson's paper, of thirty-two pages of illustrations of colonial buildings, among them fortifications and other public and private structures.

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ERNEST S. DODGE. *Beyond the Capes: Pacific Exploration from Captain Cook to the Challenger, 1776-1877*. Boston: Little, Brown and Company. 1971. Pp. xv, 429. \$12.50.

"Geography," said the eighteenth-century French explorer Louis de Bougainville, "is a science of facts" (quoted in J. C. Beaglehole, *The Exploration of the Pacific* [3d ed.; Stanford, 1966], 3). The history of geographical exploration is thus a branch of narrative history, nowadays out of fashion, consisting as it does of stories of high adventure, of the deeds of men who hazarded their lives to bring back the news of far-off lands. In this handsome book Ernest Dodge has told the story of the final century of geographical exploration in the Pa-

cific by Europeans and North Americans. Beginning with the broad general outline of Pacific geography established by James Cook, Dodge groups his explorers by nationality and treats the discoveries of nearly all the major voyages through H.M.S. *Challenger's* circumnavigation of 1872-76. (The major omissions are Russian voyages for which there are no translated accounts.) Beaglehole's book, which served Dodge as a model, follows the three-century-long quest for *Terra Australis Incognitae* until Cook's facts exposed it as myth. Unlike Beaglehole, Dodge has no thread running through his account. The result is good, old-fashioned, narrative history. The author has gone directly to the explorers' accounts (or their English translations), from which he quotes liberally without interrupting the narrative flow, paying especial attention to their ethnographic observations. These tales, some lurid, some sober, were eagerly seized upon by those who stayed at home, uneasy enough about the degree of civilization in their own societies to enjoy reading about unenlightened primitives. Commerce and politics also helped launch the sailing vessels, which, as they roamed round the world, found hitherto unknown Pacific shores. Furs from the Northwest coast of North America, *bêche-de-mer* (an edible sea cucumber) and sandalwood from the Pacific Islands, all were exchanged in China for tea, silks, and porcelains. New geographic knowledge came also from the hunt for seals and whales. The flag followed trade into the farthest corners of the Pacific, most often to survey the coasts and make them safe for commerce, sometimes, as in the expeditions of von Bellingshausen and Wilkes, out of that pure quest for knowledge which is science.

Dodge's book is largely an account of the white man's encounter with savages, told from the white man's point of view with a few comments from the author (e.g., his condemnation of the brutality of Captain Charles Wilkes, U.S.N., in the Fiji Islands in 1840, [pp. 349-50]). Nothing is seen from the natives' viewpoint (cf. H. E. Maude, *Of Islands and Men* [Melbourne, 1968]), nor is the explorers' discussed critically (cf. Bernard Smith, *European Vision and the South Pacific 1768-1850* [Oxford, 1960]). The author has reproduced in black and white fifty-eight representative plates

from the contemporary printed narratives of the voyages he recounts; his one color plate (also the dust jacket) is an inauthentic modern painting of a Royal Navy surveying vessel. Though the maps are exceptionally well drawn they are inadequate. The reader who wants to follow the explorers' tracks to all the places Dodge mentions will need a superior atlas in front of him. Dodge provides a list of sources, arranged by chapters, in lieu of footnotes. A valuable chronological listing of voyages and their commanders, followed by an index wholly of proper names, completes the book.

HAROLD L. BURSTYN

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JACQUES FREYMOND, published under the direction of. *La Première Internationale: Recueil de documents*. Volume 3, *Les conflits au sein de l'Internationale, 1872-1873*; volume 4, *Les congrès et les conférences de l'Internationale, 1873-1877*. Texts established and annotated by BERT ANDRÉAS and MIKLÓS MOLNÁR, with the collaboration of CAROLE WITZIG and LADISLAS MYSYROWICZ. (Publications de l'Institut Universitaire de Hautes Études Internationales, Number 48.) Geneva: the Institut. 1971. Pp. xviii, 668; 835.

Nearly ten years ago the general editor of the two tomes under review issued the first two volumes of documents bearing on the history of the International Workingmen's Association from 1864 to 1872. The volumes before us continue the record of the association to its end in 1877. The first series of documents gave us only the official proceedings of the meetings and the reports relating to them; the second supplements the official account with accessory material. With an eye to comprehensiveness the editors have included such documents as the minutes and reports of local and regional federations, circulars of dissident groups, articles written for the press by outstanding men, letters that were only peripheral to the large body, even manifestoes of seceded sects. Now pieces of this character are undoubtedly valuable, for many of them have been difficult to locate. Still it may be asked whether many of these addenda really belong in the collection under consideration. For example, are the articles of Marx and Engels on anarchism directly relevant to the documentary story of the International? The same question may be asked

with respect to Cafiero's long letter of June 12, 1872, to Engels, however pertinent it is to anarchist theory. And finally does the Blanquist manifesto of 1874, *Aux Communeux*, which has little bearing on the International, dovetail in this assembling of papers, especially since its authors left the association some two years earlier?

These questions are not designed to derogate from the high value of the present collection. The annotations are extraordinary. There are nearly 2,100 notes spread over 390 pages. And they are not the usual notes. Many of them are longish clarifications of sources or provide biographical data on prominent and obscure members of the separate organizations, so that we have a kind of biographical dictionary of them. Much of the material was derived from manuscript papers or police records. The editors have often cited from dossiers in the Paris Prefecture of Police, and it may be assumed that they checked their findings with other sources. I still recall the amazing fantasies in such French police dossiers as those on the International in America and on Marx, alias Williams.

The present volumes span the five years following the Hague Congress, during which two battered and moribund Internationals, the centrist and the federalist, were vying for supremacy. The one was Marxist in orientation; the other, anarchist and autonomist, with the shadow of Bakunin hanging over it. The first held two congresses, one in Geneva in 1873 and another in Philadelphia in 1876. The Congress in Geneva turned out to be a fiasco, so much so that the General Council in New York was unable to get the full resolutions, not to mention a connected story of the proceedings. The Congress in Philadelphia was the swan song of the Marxist International.

The anarchist International held four congresses, starting with the one in Geneva in 1873. Despite reports of growth in Switzerland and Spain, it too was passing out. It was torn by differences over two major questions, that of public services (which inevitably brought to the fore the divisive issues of the state and political action) and that of the general strike (which many anarchists regarded as the open sesame of emancipation). By the time of the Brussels Congress of 1874 its substance was

going to pieces. Two years later only a remnant organization met at Berne. Delegates of what was left of it gathered at Verviers in 1877 in preparation for a joint meeting with socialists at Ghent.

The editors were well advised to conclude the collection with the Ghent Congress. Though it does not belong in the series of meetings held by either of the two Internationals, it represented an earnest effort to restore a form of unity. In the history of socialism it stands as the forerunner of the Second International.

A word about the bibliographical equipment. The editors have apparently drawn on many libraries, archives, and special collections of unpublished papers. This in itself represents colossal industry. They have furthermore furnished us with lists of publications and periodicals, which, despite many omissions, form one of the best bibliographies on the First International I have seen. It is not too much to say that the present volumes will be a reservoir of original sources for historians of labor and social philosophy.

SAMUEL BERNSTEIN
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LOUISE H. HUNTER. *Buddhism in Hawaii: Its Impact on a Yankee Community*. Honolulu: University of Hawaii Press. 1971. Pp. x, 266. \$9.00.

Students of Hawaiian history, sociology, and politics will appreciate *Buddhism in Hawaii*. While its nearest counterpart, Frances Hilary Conroy's *The Japanese Frontier in Hawaii* (1953), deals with the interaction of the two dominant groups—the Americans and the Japanese—in the political, economic, and social scene and the rising antagonism up to 1898, Mrs. Hunter emphasizes the vicissitudes of the Japanese Buddhists from the arrival of the first group in 1868 through the tensions of the mid-1950s. Most of the first 141 men, with 6 women and 1 child, went to work on plantations. Forty of the most dissatisfied returned to Japan, 36 journeyed to the United States, while 37 sojourned in the Hawaiian Kingdom. Some of these were converted to Christianity by devout Protestant or Catholic missionaries. The Reciprocity Treaty of 1876 with the

United States, heralding an enormous expansion of the sugar and related industries, pointed out the need for imported labor. King Kalakaua's visit with Emperor Meiji in 1881 resulted in a special agreement with the Japanese government for the resumption of Japanese immigration, and there followed thousands of contract laborers.

The first Buddhist priest arrived in 1889 and plans were promptly drawn for a mission hall and a Buddhist temple in the heart of Puritan Honolulu. Louise Hunter treats the initial setbacks and persistent obstacles Buddhist leadership encountered and the conflict between the Buddhist community and the Caucasian community in the changing political context of Hawaii. The narrative is neatly woven into the history of the kingdom, the republic, and the territory, along with the hatred engendered by Pearl Harbor, the mistreatment of *Nisei*, and World War II, which was totally disruptive of Buddhist activities. Although these activities were revived after the conflict, there was a suppression of Buddhist youth programs.

Mrs. Hunter has written a scholarly work within well-defined limits and has made a substantial contribution to the historical literature of Hawaii. Readers of her book will acquire a better understanding of the efforts and frustrations of the spokesmen of Buddhism in the Hawaiian Islands as well as an appreciation of the contribution of Buddhism to the community. "Its teaching made its followers good citizens, good neighbors, kindly men and women." No religion could be expected to do more" (p. 209).

MERZE TATE

Howard University

BERNARD FERGUSSON. *The Trumpet in the Hall, 1930-1958*. London: Collins. 1971. Pp. 286. £2.25.

Sir Bernard Fergusson's memoirs make fascinating reading. He writes with great verve and skill, and one finishes his book with the feeling, not very common after reading the memoirs of retired soldiers, that it has certainly been worth the time. He entered the army in 1930 and retired in 1958, his career thus covering the British Army's last great war and the subsequent run-down of both that army and

the empire it served. His assessment of the strange figure of Orde Wingate, under whom Fergusson served from 1942 to 1944, is very interesting, as is his account of two tours in Palestine, first as an intelligence officer in 1937-38 and then while seconded as an assistant inspector general of the Palestine police in 1947.

The book is above all an important document on the social history of the British Army in Brigadier Fergusson's time. He came, like many British regulars of the last two hundred years, from Scottish landed stock. His father and grandfather had been soldiers. When he joined the Black Watch from Sandhurst in 1931, the minimum private allowance required was £ 250 a year, and, Fergusson records, "there were at least three subalterns with private incomes of over £ 2,000 a year, . . . and one married subaltern who kept a butler." He manages to convey, better than any military memoirist I have encountered, what a small clubby world the British regular army was. The strength of the regimental tradition is also given ample illustration. "The Black Watch was more like a religion than a regiment; I certainly always thought of myself as being in the Black Watch rather than in the Army." All of this makes his account of the years after 1945 rather poignant. Officer selection procedures change, the regimental tradition comes under attack, the "American rules . . . and American jargon" of SHAPE prove that "we, the British, were in very truth the junior partners of the Americans . . . we were no longer Equal Top Nation." The book appropriately comes to an end with the tragicomic Suez operation (during which Fergusson was a rather miscast director of psychological warfare). *The Trumpet in the Hall* is a monument—and a farewell—to the army that Bernard Fergusson knew.

RAYMOND CALLAHAN

University of Delaware

V. L. ISRAELIAN and L. N. KUTAKOV. *Diplomatiia agressorov: Germano-italo-iaponskii fashistskii blok. Istoriia ego vozniknoveniia i krakha* [The Diplomacy of the Aggressors: The German-Italian-Japanese Fascist Bloc. History of Its Rise and Fall]. Moscow: Izdatel'stvo "Nauka." 1967. Pp. 434.

This is a semipopular diplomatic history of the Axis powers from the invasion of Poland to the

capitulation of Japan. It is remarkable neither for new data nor novel interpretations. Its chief interest for American historians is likely to be simply the demonstration it provides of how readily the summarized descriptions of individual events can be woven together to support different general interpretations. It shows how independent one's view of the macrocosm can be from one's view of the microcosms.

The book is framed in the orthodox Soviet Marxist-Leninist matrix of historical assumptions. The central body of the study, however, is remarkably free of obeisances to Marx and Lenin. The introductory chapter leads up to the Nazi invasion of Poland, following the conventional Soviet interpretation of 1930s diplomacy and focusing on Munich. At each critical point here a protective citation of party documents sustains the authors. The body of the study comprises twelve chapters running from fascist diplomacy during the period of "phony war," through the fall of France, the Tripartite Pact, the attack on the USSR, Japanese aggression in the Pacific, the turning of the tide of war in Russia, to the breakdown of the Axis and the end-of-war maneuvers of Axis leadership. The center of gravity of the book is, predictably and legitimately, the outlook and decisions of Axis diplomacy regarding the Soviet Union. The short conclusion draws a moral about the futile fiasco into which imperialism pushed the Axis at this juncture of the historical process, only to produce in the long range a strengthening of the forces of socialism.

The tone of the book is that of a straightforward outline without the loose edges of uncertain or perplexing areas. A clear answer is asserted for all questions. Personalities and coincidental determinants are de-emphasized, as appropriate to the philosophic stance adopted. Stalin's name appears, but infrequently and then usually in quotations from correspondence. The scope entails more survey than intensive and exhaustive historical inquiry. When one adds the characteristic of a tale fitted to a predetermined pattern that is never really questioned, there results a semblance of superficiality that is probably unavoidable given the context in which the book was produced.

But, accepting the limitations of Soviet canons of criticism and interpretation, this is not a trivial work. The twelve central chapters are

not propagandistic; they proceed in a clear, professional succession, creating a coherent synthesis. Heavy reliance is placed upon the Documents of German Foreign Policy, 1918-45. Other prominent sources include the records of the International Military Tribunal for the Far East and the Nuremberg Trials, U.S. Pearl Harbor inquiry materials, and the Soviet documentary collections of the war years. German and Italian memoirs have been used, and there are occasional references to Western secondary literature. The overall impression left is of a serious historical effort by competent scholars that pushes against rather stringent ideological constraints upon historical scholarship.

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MARK W. ZACHER. *Dag Hammarskjöld's United Nations*. (Columbia University Studies in International Organization, Number 7.) New York: Columbia University Press, 1970. Pp. 295. \$7.00.

DAVID A. KAY. *The New Nations in the United Nations, 1960-1967*. (Columbia University Studies in International Organization, Number 8.) New York: Columbia University Press, 1970. Pp. xiii, 254. \$10.00.

These two books, similar in weight and length, concerned with the same international organization, and published in the same series by the same publisher in the same year, could hardly be more unlike. They spring from very different investigative traditions, aspire to very different kinds of knowledge, ascribe historical momentum to very different sources, employ very different research methodologies, and arrive at very different types of conclusions, thus leaving the reviewer with very different assessments of their utility.

The Zacher monograph is best described as intellectual history. Based on a close examination of the writings and pronouncements of the United Nations' second secretary general, Zacher seeks to reconstruct the strategies and tactics employed by Hammarskjöld during his eight years in the office that are relevant to the peaceful settling of disputes, controlling the use of force, promoting arms control and disarmament, and building a more peaceful world order. In so doing Zacher implicitly ascribes historical causation to the will of individuals and relies exclusively on Hammarskjöld's views as

evidence of the way in which leaders make rather than reflect the forces of history. Thus in the end Zacher is content to conclude that Hammarskjöld was an important figure in the fledgling organization over which he presided.

The Kay monograph, on the other hand, is most appropriately characterized as behavioral analysis. Based on a close, quantitative analysis of roll-call votes cast in the UN General Assembly by new (post-1960) members, Kay attempts to discern the patterns of organization and influence through which the new nations express their concerns and move toward their goals. His assessment focuses on the participation of the new nations in the consideration of decolonization, human rights, economic development, East-West differences, and organizational issues over several sessions of the General Assembly. The result is an implicit ascription of causation to the behavior of large collectivities and the political processes through which they interact. The patterns of influence uncovered by the analysis leave little room for individuals as causal agents, with the result that Kay's conclusions deal with the capacity of the new nations and the United Nations to harmonize their interests and achieve their respective aims.

For me the Kay volume has considerably greater utility than the analysis by Zacher. Both the history and the future of the United Nations strike me as lying in the actions and reactions of many people in many groups in many situations. The secretary general is not irrelevant to those interferences, and certainly every facet of the United Nations should come under scrutiny, but in the long run, if not in the short run as well, the course of international history seems likely to be shaped mainly by the convergence of forces in which the talents, aspirations, and perspectives of particular individuals are of relatively minor importance.

JAMES N. ROSENAU

Ohio State University

ANCIENT

RONALD JESSUP. *South East England*. (Ancient Peoples and Places, Volume 69.) New York: Praeger Publishers. 1970. Pp. 273. \$8.50.

The three southeastern counties of England—Kent, Surrey, and Sussex—contain some

of the most important archeological sites in the country, since they were the gateway for invaders from the Continent. Ronald Jessup's work provides a useful survey of prehistoric sites and artifacts uncovered in the region. He begins with the topological background then covers chronologically the findings from various cultures from earliest hunters and food-gatherers to the megalith builders, Bronze Age peoples, and Celts, down to the Romans. The longest chapter by far is the one on Roman Britain. The book contains seventy-five plates, a number of drawings, lists of sites and museums, and a full bibliography. Unfortunately there are no footnotes.

The absence of notes raises the question of the readership at which Jessup's book is aimed. An absence of any historical background comparable to the topological background will confound readers who are not specialists in prehistoric Britain, as will the use, without providing definitions, of technical terms such as "Hoxian Interglacial Period" or "Hallstatt culture." Nonspecialist readers will regret the scarcity of "intimate sidelights on human life" that archeological artifacts can offer. Somehow the author's imagination and literary skill are inadequate to bring the prehistoric peoples back to life for the reader. Perhaps he tried to do too much in too short a book. If he had omitted the Roman occupation, more familiar to readers, he might have had greater success with the earlier, less familiar periods.

Perhaps Jessup is aiming his work only at specialists in prehistoric archeology. Even these readers may be disappointed, since they will find few accounts of new finds, but mainly summaries of excavations already described in British archeological journals. Yet the book will earn a place on the bookshelf of the serious student of the archeology of prehistoric Britain, because it does draw together in one convenient volume descriptions of sites that otherwise would be scattered in dozens of journals and local publications.

RALPH V. TURNER

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I. E. S. EDWARDS *et al.*, editors. *The Cambridge Ancient History*. Volume 1, part 2, *Early History of the Middle East*. 3d ed.; New York:

Cambridge University Press. 1971. Pp. xxiii, 1058. \$23.50.

Volume 1, part 2 of the new *Cambridge Ancient History* takes the story of the Middle East from the dawn of civilization forward to about 1750 B.C. Of its seventeen chapters, three are devoted to Egypt (Early Dynastic, Old Kingdom, Middle Kingdom), five to the lower Tigris-Euphrates valley (Protoliterate to just before the Age of Hammurabi), two to Anatolia, three to the Syria-Palestine area, and one each to Assyria, Persia, and the Aegean world along with a final chapter that deals with Indo-European origins and the impact of the Indo-European invasions upon the Middle East. Contrary to the impression of uneven distribution of bulk that these mere statistics might convey, the net result of this division of chapters is a very satisfactory balance of the material.

Nearly half a century, forty-eight years to be exact, separate this volume from volume 1 of the old edition, and one cannot resist the temptation to make certain comparisons:

Volume 1 of the first edition (1923) ran just over 700 pages and contained 17 chapters, the work of 9 contributing authors. The first volume of the new edition has been issued in parts 1 and 2 of which the latter, the one under review here, itself has 1058 pages, 17 chapters, and a total of 18 authors. In the older edition the general subject matter of the new volume 1, part 2 was covered in 400 pages. As for the bibliographies accompanying the chapters, 36 pages sufficed for volume 1 of the *editio princeps*; in this second part alone of the new volume 1 the bibliographies account for 112 pages.

From this it is not difficult to arrive at the conclusion that our fund of knowledge has increased not merely in bulk but also with regard to certain regions (Elam, Anatolia, Syria-Palestine) of which our view was a bit hazy fifty years ago. Moreover, where in the old edition T. Eric Peet did most of the chapters on Egypt and Stephen Langdon on Mesopotamia, we find that no fewer than six authors provided the new material on these same topics. One obvious conclusion to be drawn from this is that specialization has grown apace in the past fifty years.

On the other hand, one may ask the question "How new is this new edition?" It is

called the third, and it is dated 1971. This means that its chapters, issued as separate fascicles mostly between 1961 and 1965, constituted edition two and that there was some revision before they were put together into a single volume. Careful examination does not show this revision to have been extensive except that new bibliography has been added including a few items as recent as 1968. It therefore seems fair to state that the present volume pretty much sums up scholarly knowledge and opinion as it was in 1965. The world does not stand still, and there have been some changes since that time.

These remarks are intended more as a caveat than as criticism. The new edition has been an enormous undertaking, and its preparation consumed years. Seven of the eighteen authors are now dead—most recently, C. J. Gadd in 1969, and R. DeVaux and Carl Blegen in 1971. As everybody knows, books of this sort are longer in press nowadays than used to be the case. In the edition of 1923, the bibliography included works published as recently as 1922. More important perhaps was the fact that the first edition appeared right after World War I during which scholarly activity and archeological field work had virtually come to a standstill and thus given the authors a chance to survey and judge the condition of a temporarily static field. No such opportunity was afforded the authors of the new chapters; they could look back upon their journey and describe the scenery at the point where they had left the train, but the streamliner on which they had been traveling was already miles away.

The present volume, however, is open to one general and serious criticism, and the blame originates with the excessive specialization imposed upon scholars by the growth and proliferation of the field. The old edition was intelligible to the nonspecialist historian and even the general reader, but most of the new chapters do not have these virtues. On the contrary they were written by specialists for other specialists to read. They fail to provide the overview that the nonspecialist has a right to expect in a volume of this kind, and he is in no position to judge the worth of the section he may be reading. It may be of some consolation to him to learn, however, that a certain proportion of this material, timely though it once was, will

not stand the test of time and will be thrown into the discard.

An extreme, though not the only example, of topicality is afforded by James Mellaart's chapters (18 and 19a) in which an interpretation of material from a few assumed "type sites" in a virtually unexplored area is marshaled to construct hypotheses that could be negated by a few excavations on new sites in the same region. Surely our experience with Mesopotamian pottery from "type sites" as a clue to change and the identity of peoples should warn against easy acceptance of fragmentary evidence and vague similarity as historical criteria for much of anything.

Jaded perhaps by familiarity and disappointment with the fascicules that constituted the second edition, I cannot muster a great deal of enthusiasm for edition three. I have expressed my sympathy for the editors in regard to some of their problems but cannot condone a policy that allowed the authors to write for one another rather than the audience for which the *CAH* was originally intended.

TOM B. JONES
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MORTON SMITH. *Palestinian Parties and Politics That Shaped the Old Testament*. (Lectures on the History of Religions, sponsored by the American Council of Learned Societies. New Series, Number 9.) New York: Columbia University Press. 1971. Pp. viii, 348. \$9.00.

The thesis of this important book is that the Hebrew Bible, the "Old Testament," is the product of the victor in a prolonged conflict between two parties within Palestinian Israelite religion. "The syncretistic form of the cult" honored Yahweh along with local deities; it "was spread widely by Israelite deportation and/or emigration from the eighth century on, and evidently secured considerable adherence from gentiles." The Old Testament "representation of the Israelites as constantly in conflict with the demands of their own religion" is in fact an attack upon this "syncretistic piety" by the other side, the stricter "Yahweh alone" party; this group (at first a minority of the Israelite population) gains the support of the Persian court during the exile in the sixth century.

With the success of Nehemiah as governor in

Jerusalem for the Persians, the Yahweh-alone party evolves into the "separatist party," going beyond opposing official syncretism (which had come to an end) to even more stringent demands, that is, that all who are "polluted" by the worship of other gods be barred from the Jerusalem temple and from marriage with Judeans. The separatists are supported by the Jerusalem masses and the levites; they are also apparently pro-Persian. (A mark of their success is the gradual emergence of the institution of the synagogue, which favors laity over priesthood, community assembly over temple service, and prayer in place of sacrifice; in the gentile world, too, at this time sacrifice is giving place to more "spiritual" forms of worship.) The assimilationists, on the other hand, include nearly all the priests and the gentry of Jerusalem, Judaea, and the surrounding area; they favor Egypt.

The position of this separatist Yahweh-alone group was "enhanced by the success of the literature which it produced or edited (most of the books now in the Old Testament) and by the traditional prestige of Jerusalem. Consequently the adherents of the old, popular form of the cult gradually assimilated their claims and practices to those of Jerusalem, and in effect were converted to the Yahweh-alone position." The Maccabees greatly further this process, which becomes "substantially complete" with the rabbinic Judaism of the late Roman Empire.

Smith is applying a method known already in other areas of religious studies, the identification and analysis of the various competing parties within a larger religious group; the result is a fascinating description of the ebb and flow of social, political, and religious forces particularly in the period (to which five of the seven chapters are devoted) between the fall of Jerusalem in 587 and the rise of the Maccabees in the second century B.C. He is skilled at relating political and religious developments in Palestine to those in other parts of the ancient world; his discussions of "Hellenization" and of the social stratification known from Amos and Hesiod, for example, and his presentation of Nehemiah as a classical tyrant should be read carefully by all who are interested in the Greek world.

Seldom is the Old Testament treated thus:

not as normative "sacred history," not as data for social scientific analysis, but as the product of an ancient religion, to be examined in its broad historical context as any other ancient religion would be. The emphasis on "parties" will surely be questioned: does this religion really fall so neatly into a left and right wing? Did the variations in Persian or Egyptian political influence have such strong effect upon it? If syncretistic Judaism was so common, would it really have been possible to obliterate it almost completely? But Smith presents a well-reasoned, well-documented position (nearly half the book is taken up with bibliography, indexes, and footnotes); the burden of proof is now with those who would debate him.

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A. M. SNODGRASS. *The Dark Age of Greece: An Archaeological Survey of the Eleventh to the Eighth Centuries BC*. Edinburgh: University Press; distrib. by Aldine-Atherton, Chicago. 1971. Pp. xxiv, 456. \$27.50.

The subtitle of this volume is far too modest. Professor Snodgrass has indeed surveyed in masterly fashion an impressive quantity of diverse and widely scattered physical remains from the period 1100–700 B.C. More than 350 sites are listed in the index, and these extend from Epirus and Macedonia in the north to Crete in the south, from Cyprus and the Asia Minor mainland in the east to the western colonies. But this is much more than a survey of the archeological evidence, for he interprets this material as he moves along, especially in the final two chapters devoted to external relations and internal development. Before reaching his own conclusions, moreover, he summarizes other interpretations that have been offered so that the reader has a good summary of the state of modern scholarship before him. The volume is logically laid out: the author considers the material in its chronological sequence and regional context, and this allows us to see both chronological development and regional diversity more clearly than has previously been the case. The text is illuminated by 138 well-placed illustrations of generally excellent quality.

Much of what Snodgrass has to say has been

said before. His discussion of the pottery and the chronology derived from it is based largely on the works of Vincent Desborough and J. Nicholas Coldstream. His overall view of the Dark Age as a period of stagnation and isolation followed by the renewal of overseas contacts and several centuries of slow but steady progress until the pace of change accelerates markedly about 750 B.C., is generally similar to that advanced by Chester G. Starr. Yet the author is no slavish follower of his predecessors; he suggests numerous modifications of his own in matters both large and small. His chapter on the metal objects may be the most comprehensive discussion of this often neglected body of evidence that has yet appeared. But it is primarily in his interpretation of the archeological material that the volume will generate discussion, for many of his conclusions are original and provocative.

While Snodgrass believes that the period justly deserves the designation Dark Age he does not share the opinion that it was inaugurated suddenly by a wave of invaders from Central Europe. He finds no evidence in the physical remains that an intrusive population element was responsible for the widespread destruction that took place throughout Greece toward the end of the Bronze Age. To arrive at this conclusion he regards the presence of intrusive metal objects as insignificant when viewed against the evidence for continuity, particularly in pottery styles; and following Deshayes he rejects Desborough's notion that single burial in cist tombs is really a new phenomenon and argues that it was a carry-over from Middle Helladic times that never completely died out in the Mycenaean period. The destruction was caused, he believes, by migrating Thessalian, Boeotian, and Dorian Greeks, who were largely indistinguishable in their physical culture from the Mycenaean Greeks and who, prior to their migration, lived on the outer fringes of the Mycenaean world. Snodgrass sees Athens clearly in the vanguard throughout the Dark Age; regions outside Athens he classifies as progressive, imitative, and isolated. Athenian leadership, he suggests, may have derived from the political unification of Attica perhaps as early as the late tenth century. He would limit the period during which Greece was isolated from the outside world to hardly more

than three quarters of a century between 1025 and 950 B.C. He notes that it was precisely during this period that iron artifacts begin to turn up in proportionately significant numbers in the more advanced areas of Greece, but after 950 there is a reaction against the use of iron for certain objects. He concludes that the Greeks began to work iron not because it was inherently superior to bronze but because their source of bronze ores had been cut off.

This is only a sampling of what awaits the reader of this exciting volume; it contains far more than can be dealt with here. While over-generalization from isolated finds is not always avoided, and a few slips have inevitably crept in, these are minor when viewed against the whole. Professor Snodgrass has not solved all the problems of the Dark Age, but he has risen far above the tradition of the ancient Greeks and its modern expounders. There will be many who find it difficult to accept some of his conclusions, but even his critics will have to acknowledge that he has shed much light on the Greek Dark Age. As a result, many of our fundamental beliefs about the period will have to be thoroughly re-examined.

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Minneapolis

HUGH LLOYD-JONES. *The Justice of Zeus*. (Sather Classical Lectures, Volume 41.) Berkeley and Los Angeles: University of California Press. 1971. Pp. xiv, 230. \$8.50.

This volume presents a provocative if highly personal polemic against the current direction and emphases of work in Greek intellectual history from its beginnings with the Homeric poems through the philosophy of Plato in the fourth century B.C. Basically, as his title indicates, Lloyd-Jones attempts to show that the Greeks conceived of Zeus as a just god who rewarded good and punished evil; that the Greeks as early as the *Iliad* had a concept of justice (*dike*), of right and wrong; and that they conceived of the cosmos as an ordered and moral entity.

There is merit in this point of view; I have sympathy for it and would agree at many points. On Pindar, for instance, Lloyd-Jones

appears to agree with me on the poet's theology ("Pindar's Second Pythian Ode," *American Journal of Philology*, 84 [1963]: 377-89) although he does not cite me. His work, however, largely fails because of its highly contentious style and because of a certain selectiveness of argumentation.

Lloyd-Jones develops each line of reasoning by arguing against a previous interpretation and in particular against the anthropological and psychological approaches of E. R. Dodds and A. W. H. Adkins. This is distracting in itself; but, more seriously, I feel that Lloyd-Jones has missed the general intent of his opponents and distorts the essential nature of their work. In his chapter on Sophocles he builds his argument in reaction to an article by Dodds ("On Misunderstanding the *Oedipus Rex*," *Greece and Rome*, 13 [1966]: 37-49). Dodds's intent was to criticize classical education at Oxford by analyzing the kinds of interpretation given by the undergraduates of the *Oedipus Rex*. His own interpretation is based on that of B. M. W. Knox (*Oedipus at Thebes*, [1957]). Nowhere does Lloyd-Jones cite Knox. It is certainly permissible to disagree with Knox's work but not to ignore it.

In his chapter on Homer, Lloyd-Jones shows limited understanding of the implications of oral technique. Greater understanding would in fact have buttressed his argument. Specifically Adam Parry's article "The Language of Achilles" (*Transactions of the American Philological Association*, 87 [1956]: 1-7) would have supported his argument about the inadequacy of deriving conceptual structure from the study of individual words.

Lloyd-Jones's knowledge, learning, and sympathy for the Greek struggle at the beginnings of Western civilization are impressive. One trusts that in the future he will rid himself of the contentious style and present us with a positive treatment of Greek thought. Classicists and ancient historians should read this work, generalists only if they are willing to read all the works Lloyd-Jones attacks.

I must in conclusion complain about the work of the University of California Press. The type is handsome and readable although the Greek, used sparsely, is badly aligned. But the notes collected at the back and numbered separately for each chapter are not provided

with any headings. Consequently the search for a specific footnote is tedious and bothersome.

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G. E. R. LLOYD. *Early Greek Science: Thales to Aristotle*. (Ancient Culture and Society.) New York: W. W. Norton and Company. 1970. Pp. 156. \$6.00.

JACK LINDSAY. *Origins of Astrology*. New York: Barnes and Noble. 1971. Pp. xii, 480. \$12.75.

In his slim, clear, and cogent volume G. E. R. Lloyd presents the development of Greek science from what we can see of its beginnings through Aristotle and his most immediate successors. His work is nothing less than an excellent analysis of nonethical philosophy in this period. He includes medicine and gives much attention to astronomy but rightly shows that the epistemology and cosmology of the philosophers are central to our understanding of the problems. After the very fact of the formulation of the questions that science must answer their major achievements were the establishment of empirical research and of the importance of mathematics for understanding natural phenomena. There is a useful consideration of the degree to which experimentation and observation were employed, and sometimes misapplied, and of the degree to which they were either irrelevant or beyond the reach of the scientists of the time. For historians the discussion of the social conditions under which Greek science began and Greek doctors and philosophers worked will be of particular interest. Lloyd's comments are perceptive, and he is not tempted to go beyond the very limited evidence we have. One may regret that he has virtually nothing to say on technology—marginal to what he makes his central subject. (Technology is generally neglected in the study of Greek science as shown by the reference to the unsatisfactory work of Forbes in the bibliography as well as a pious gesture toward Lévi-Strauss for the primitives.) But more regrettable is that the limitation of the period prevents us from following Lloyd on to the great era of Hellenistic science. To lament the price of the book is only to say that one would like to see the widest use of it.

Lindsay's *The Origins of Astrology* is in-

tended as a companion to his *Origins of Alchemy* (1970) and is generous in pages and in information, culled through wide and generally intelligent reading. The footnotes and bibliography, however, are so compressed that they are virtually useless; their purpose seems to be to assure the reader that the author has indeed read the works not to help the reader to go to them himself. Lindsay overwhelms the reader with the quantity of data, and undoubtedly his work will be a rich quarry for many a college term paper. He tries to keep the main lines of development before us and does not shirk general interpretation. He tries to make a case for Democritus as a great popularizer of Babylonian astronomy among the Greeks, but against this and for all questions of the relationship of *Early Greek Philosophy and the Orient* (1971), see M. L. West's new book by that title. Lindsay sees astrology, as he saw alchemy, as representing a whole view of the world in which the events of the heavens and earth are interrelated. One may demur at his notion that it flooded into Greece as a result of disillusionment with democracy after 400 B.C., but the concluding chapter in which astrology aligns itself with post-Newtonian physics gives a hint at the passionate enthusiasm that has propelled the author and the weary reader through so much dreariness.

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CHESTER G. STARR. *Athenian Coinage, 480-449 B.C.* New York: Oxford University Press. 1970. Pp. viii, 95, 26 plates. \$10.50.

The years from 480 to 449, which witnessed the development of the Athenian Empire, undeniably represent an "important but ill-lit period" in Greek history. Since contemporary evidence is extremely scanty for these years, the great abundance of Athenian coinage might seem an obvious source of illumination. But as Starr correctly observes, this material is "commonly treated as a wasteland," subject to "an amazing amalgam of views often illogical, even inconsistent," passed on without serious analysis. The consequence has been "the apparent destruction of any historical value for the coinage." Starr's object is to study the coinage for its potential value as contemporary evidence,

and he begins where any solid approach must: with an attempt at classification and chronological arrangement.

An important segment of the book (chs. 2-4) is devoted to a description and systematic classification of the coinage into five groups covering 480-449, and general remarks about the vast but relatively unchanging bulk of material from 449 to the end of the century. The principles of arrangement depend largely on developments in style and particularly in design. This method seems the most feasible, since classical Athenian coins bear no date or magistrate's mark; the hoard evidence is too scanty and uncertain to permit sequential arrangement on this basis alone; and the basic elements of the coinage, Athena's head on the obverse and the owl, olive-twigs, crescent moon, and ethnic on the reverse, manifest numerous minor variations and stylistic developments. Treatment of each group includes a description of common characteristics; tabular catalog with detailed observations, weights, references to publications or collections and to plate illustrations; and a discussion of dating criteria. Starr argues cogently on the basis of the Erechtheion hoard (ca. 480) and general historical considerations (widespread Persian devastation of Attica, the need to resupply the mines with a labor-force, etc.) to a date of approximately 477 for the resumption of minting activity with a new, post-Persian War coinage (distinguished by the crescent moon and the inclusion of olive-leaves on Athena's helmet). Detailed stylistic analysis for subsequent groups follows. Two further chronological lynchpins are the Greek victory of the Eurymedon about 467 (to which is associated the brief and unusual issue of decadrachms) and the events of 449 (Peace of Callias, Congress Decree, and initiation of Pericles' building program with League funds), which necessitated the mass standardized issues that dominate the later fifth century. Starr estimates that these issues represent ninety per cent of coinage, 480-ca. 410.

This new classification leads to several interesting possibilities, including the reassignment of several issues apparently misdated in Seltman's *Athens: Its History and Coinage* (1924); new emphasis on the impoverishment of Athens prior to 449; the suggestion that the famous Currency Decree may have been a proposal of

the Treasurers of Athena intended primarily to simplify their tasks, rather than a mark of Athenian coercion and imperialism; and the probability that Athenian coinage came to be a standard medium of exchange only after Athens had monopolized Aegean silver.

Stylistic arrangement can be impressionistic and arbitrary, but this classification is a clear, internally coherent system based on careful analysis of the numismatic evidence and broad historical perspective. Its weakest link is the terminus of 449, established by reference to historical events that are hotly debated by historians. While the argument cannot be conclusive (as Starr recognizes) it is very persuasively presented. The book is a lucid and valuable contribution demonstrating how historians and numismatists can combine their special areas of study to broaden knowledge of classical Greece.

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THOMAS FISCHER. *Untersuchungen zum Partherkrieg Antiochos' VII. im Rahmen der Seleukidengeschichte*. Tübingen: the Author. 1970. Pp. ix, 124. DM 12.

This is a typical German doctoral thesis in the classical history field. The author, a student of Hermann Bengtson of Munich, has collected all of the sources on the eastern aspirations of the last Seleucid ruler to attempt a reconquest of Iran, which had fallen away from the Hellenistic dynasty. His conclusions are that Antiochus VII led two expeditions, not one, against the Parthians in the period from 131 to the spring of 129 B.C. when he was killed in Parthylene instead of in Media, as most scholars have supposed. Further the Seleucid army remained intact and was again led against the Parthians by a son called Seleucus until Demetrius II usurped power from his nephew. The real catastrophe to the Seleucid army came under this Seleucus and not under Antiochus VII.

Since the classical sources are so fragmentary the historian of this period is free to speculate on the discrepancies in them. Furthermore, there are no cuneiform tablets from Mesopotamia to check the chronology of the events of 131-130 B.C., and even the dates are open to dispute. Since the sources indicate that Anti-

ochus VII with a small group of followers was surprised by the Parthians and killed, Fischer assumes that the Seleucid army must have remained intact and was then led by Seleucus until he was defeated and captured. This is possible but we simply do not know. Likewise, because one source says Antiochus VII invaded Parthylene instead of Parthia, the author assumes that the Seleucid king invaded the homeland of the Parthians. But all the sources are much later than the events, and one should not count on a fine distinction in the sources rather than a not unnatural *lapsus*.

I can bring only one piece of evidence to counter Fischer. If Antiochus spent two winters, 130 and 129 B.C., on the Iranian plateau, the absence of his coins from the mint of Ecbatana (Hamadan) is difficult to explain. Then one would speculate otherwise than the author, but the end result was the loss of Iran forever to the Seleucids.

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DAVID STOCKTON. *Cicero: A Political Biography*. New York: Oxford University Press. 1971. Pp. ix, 359. \$11.25.

As consul in 63 B.C. Marcus Tullius Cicero managed to rescue Rome from the conspiracy of Catiline, an episode comparable to Everett Dirksen outfoxing Aaron Burr. An orator of great skill, vast ambition, and towering conceit (*O fortunatam natam me consule Romam*) Cicero was a political opportunist of middling ability and an eloquent spokesman for more powerful men. He also wrote a number of treatises, crammed with borrowed learning and banalities that impress clergymen as profound, but at least he was no Bryan—though an augur, Cicero debunked divination. It is regrettable that the self-made image of Cicero has always been admired by the type of mind that confuses American congressmen with statesmen. However, Cicero's voluminous correspondence reveals him and his society with such devastating frankness that Carcopino could argue that Augustus published it in order to discredit the old order. So much is known of the real Cicero that Professor Stockton can only make him look commendable by comparing him with a really repulsive character, Brutus, who rammed de-

crees through the Senate for venal personal gain and yet murdered Caesar for being "above the law." Stockton's book is a political biography aimed at undergraduates and general readers and as such is not bad, but specialists will balk at some debatable points in the first half. Despite disclaimers, the author implies that there was "a party of reform" in Republican Rome and that it was the Populares.

The second half of the book is much better and more sensible in its treatment of men and problems. What is more, Stockton writes very well—for example, Cicero "was the archetypically conservative with a small 'c.' Politics fascinated and absorbed him: the day-to-day play of affairs, the gossip, the intrigue, the formal pageant, the clash and color of personalities. Policies bored him." Good prose is no small virtue at a time when much historical writing is pedestrian and jargon-ridden, probably because of the profession's penchant for mediocrity in thought and expression. Stockton's epitaph on Cicero is superb: "His immense success as consul seduced him into entertaining ideas above his real station, leaving him with the appetite for a controlling position without the basic means to satisfy it, either in the shape of widely based political support and connections or—to be honest—the intellectual and moral equipment for such a role. For the next twenty years, 'the glory of the Nones' was to hang around his neck like a millstone dragging him further and further down." To his credit, Stockton has not told a tragic tale but an honest and instructive one with heuristic value for fields other than Roman history.

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MEDIEVAL

R. W. SOUTHERN. *Medieval Humanism*. (Torchbook Library Edition.) New York: Harper and Row. 1970. Pp. x, 261. \$9.00.

The twelve essays collected in this volume, some of them now printed or printed in full for the first time, are grouped under four headings: biographical sketches of Bede, St. Anselm, and Meister Eckhart under the heading "Three Stages of European Experience"; essays on me-

dieval humanism, the school of Chartres, Abelard and Heloise, and Peter of Blois under "Aspects of Humanism" (the heart of the book); discussions of England's first entry into Europe in 1066 and on England's place in the twelfth-century renaissance under "Europe and the 'Other World'"; and biographical pieces on Ranulf Flambard, Henry I, and Pope Adrian IV under "Three Types of Practical Wisdom." They focus on themes that have concerned Professor Southern for some years, one of them, indeed, being "an extensive revision" of the much admired paper on Ranulf Flambard originally published in 1933 and first written, the author tells us, at the suggestion of Professor V. H. Galbraith to whom the volume is dedicated. As their titles suggest, these essays concern English history as well as Continental, the history of practical affairs as well as that of spiritual and intellectual life, broad-gauge problems of historical interpretation as well as detailed questions of manuscript provenance and textual criticism. Though I must confess to having experienced a little difficulty in discerning the "unity of theme" to which the author aspires, the "unity of treatment," on the other hand, is readily apparent. For if the subject matter of these essays reflects very well the impressive range of Professor Southern's interests, the way in which he handles it also exhibits throughout that unusual combination of bold originality with steadiness of historical judgment and reflective sensitivity to the feeling of an era that we have come to expect of his work. Nowhere is this more evident than in the numerous sections of the book concerned with biographical matters: in less than twenty pages, for example, he does more to render accessible to the modern reader both the letters of Abelard and Heloise and the personalities concealed behind them than has many a lengthier analysis.

Admiration, then, is very much in order. This is a distinguished volume. But admiration does not (and, I suspect, will not) necessarily imply agreement. There is much to provoke in these essays. Many, no doubt, will be so provoked, and most, I would guess, by the two central contributions on medieval humanism in general and on the School of Chartres in particular. For me, the latter of these, with its fascinating debunking of the special distinction

traditionally accorded to the twelfth-century school at Chartres, carries conviction. But the former, with its redefinition of humanism, its depiction of Renaissance humanism as a "product of disillusion with the great projects of the recent past" (p. 60), and its insistence that "the period from about 1100 to about 1320" was "one of the great ages of humanism in the history of Europe: perhaps the greatest of all" (p. 31), however golden its vision, raises as many questions as it answers. The academic or literary view of humanism as a specific program of studies may well have its roots in the Renaissance era rather than in the conditions of the twelfth century itself, but it is no more remote from those conditions than any other view—including, perhaps, that of Professor Southern himself. It retains, moreover, the added (if fustian) advantage of providing us with an appropriate label wherewith to distinguish the approach to learning characteristic of the twelfth-century schools from that more technical and specialized approach which came to dominate in the universities of the thirteenth century. But these are big issues, and if it is the merit of this book to have raised them with such force, it is also its charm to have succeeded in doing so with such urbanity.

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JOHN BEELER. *Warfare in Feudal Europe, 730-1200*. Ithaca: Cornell University Press. 1971. Pp. xvi, 272. \$7.50.

Professor Beeler's book has the merit of brevity and readability. The scope of his essay, for that is what it essentially is, is enormous both in time and space. The chapters deal with Carolingian Europe, the early Capetians, Norman Italy, the Norman Conquest, crusader Syria, southern France and Christian Spain, central and northern Italy, and Germany. Moreover, he writes almost as much on the politics of war as on warfare itself. He takes an original approach, stressing the role of nonfeudal elements, of sieges as opposed to battles, of naval support, and of the terms of obligation and contract. In this he corrects traditional misunderstandings in the direction pointed by recent monographic and periodical literature. It is nevertheless difficult to assess the value of Dr.

Beeler's findings on any particular issue, as his book is practically devoid of critical apparatus. This also means that the book is of limited value to scholars, as any serious student would have to search unaided for the monographs, articles, and sources that underpin the author's findings. I will take a single example, the Normans in Sicily, as a test case. Beeler adds nothing to the material in Chalandon's monograph, which in turn (as far as the conquest of the island of Sicily is concerned) is based almost entirely on a single source, the chronicle of Geoffrey Malaterra. What Beeler gives is a brief, selective summary with stress on those elements that general accounts have usually underestimated or ignored. For the most part exact dates and details are eliminated, though, per contra, when seeking to establish a point the author will occasionally make an excursion into precise reconstruction. References to original sources, which are extremely scarce, tend to be in the form of such phrases as "it is related that . . .," and so on.

Some aspects of the subject receive little or no attention. Examples are the Peace and Truce of God, the use of parish levies, and the nature of the siege weapons employed. On the other hand, the author is trustworthy and properly cautious in dealing with numbers and composition of forces.

The only way to write about warfare in a period almost entirely lacking in record sources is by intelligent estimates of the reliability of a few chronicles written by monks remote from the business of war. Hence a major work on the subject would bristle with critical analysis and references. As it is, we have to accept Dr. Beeler's book largely on trust, an unhappy situation for the reader. It is my opinion that the essay here presented could have remained a reliable short survey, as clearly intended, without such a wholesale sacrifice of the scholarly references and criticism of sources usual in a revisionist monograph.

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Il monachesimo e la riforma ecclesiastica (1049-1122): Atti della quarta Settimana internazionale di studio, Mendola, 23-29 agosto 1968. (Pubblicazioni dell'Università Cattolica del Sacro Cuore, Contributi, Third Series, Varia,

Number 7.) Milan: Editrice Vita e Pensiero. 1971. Pp. xvi. 540. L. 10,000.

This volume contains the proceedings of the fourth study week in the series organized at La Mendola by the Catholic University of the Sacred Heart in Milan. This triennial conference, like the similar gatherings at Todi and Spoleto, attracts a large and distinguished body of medievalists, and the impulse and enrichment given to the studies concerned, together with the widening of horizons and interests resulting from the contacts among scholars of many nations, cannot easily be overestimated. To those who were not fortunate enough to be present this publication will be a revelation of the wealth of scholarship that is currently being expended on monastic history. In this field the French and Belgian contribution has long been recognized, but the Italian element has increased very considerably since World War II. Professors Capitani, Fonseca, Tabacco, and Violante, together with Dom Gregorio Penco, are only five among a regiment of students of the first class engaged in monastic history. Germany and Austria have never lacked representatives, but it is good to see scholars from Central Europe, Poland, and Czechoslovakia. Even so, the roll call of La Mendola has several lacunae. Spain is not represented in strength, there are many other Cistercian and Benedictine medievalists besides Dom Jean Leclercq and Dom A. Dimier, only two English historians were present, and America had only a single, albeit a notable, representative.

The topic that binds together the individual items of the present collection is unquestionably both important and fertile. Indeed, it might almost seem to have proved itself at once too manifold and too superficially familiar to give birth to a well-proportioned, comprehensive, and deeply probing survey comparable to that of the previous conference on hermit life in the West. After all, monks and monasticism were at the very heart of the Gregorian reform; they were at once its major agents and the primary objects of attack in its prehistory. Every medievalist on a degree course could be expected to write an essay on the subject. This may explain the absence from this volume of any study of the great monastic personalities, such as Damiani, Humbert, or the Cluniac popes, and any discussion of the mutual inter-

action of monasticism and reform, but room might well have been found for an exposition of the programmatical monachization of the entire Church, the people of God, their worship, and their piety, which was undoubtedly a feature of the epoch and one that remained to provide a target for the Fathers of Vatican II.

Having said so much, it is time to turn to the individual contributions, which are too numerous to receive more than brief mention. The admirable opening discourse by Professor Fonseca comes nearer than any other piece to giving a survey of the whole field, and his footnotes give a fair picture of the work that is being done. Dom Grégoire of Clervaux outlines and discusses the share of Monte Cassino in the early stages of the papal reform. There is room here for a major work of scholarship that would exhibit this great abbey in its golden age, its art, its learning, and its relations with Rome and Byzantium. One short episode in its life is studied by Professor Wolasch of Fribourg, that of the papal election of Nicholas II. Professor Marselli (Rome) then provides a lengthy study of the Carthusians and a shorter one of the Cistercians. Both he and a later lecturer stress the significance of Guigo I in the history of the Charterhouse, a significance comparable to that of Stephen Harding in the history of the white monks. Professor Tabacco (Turin) then gives a clear and well-documented review of the establishment of monastic foundations by the bishops; this is followed by a review by Professor Christopher Brooke (London) of the part played by kings, especially English kings, as patrons. Professor Kloczowski gives a very welcome account, with a bibliography of Polish works, of the plantation and development of Benedictine monasteries in Poland, processes begun by St. Adalbert (d. 997), fostered by kings and bishops, largely destroyed later, and reconstituted by Duke Casimir and Boleslav the Bold, largely with the aid of monks from Bavaria.

We then pass to the realm of the mind and spirit. Dom Grégoire (Clervaux) and Professor Mollat (the Sorbonne) discuss monastic poverty and see the monks, as corporate possessioners, still aiming at the virtue of personal poverty, the way of life known to later Franciscans as *usus pauper*, still regarding alms and hospitality, rather than assimilation, as their duty to-

ward material poverty, though there were many critics preparing the way for St. Francis. Dom Anselm Dimier's (Scourmont) chronological list of monastic buildings, with short comments and dates, will remain as a mine of reference, though it must have been somewhat overwhelming as a lecture. Professor Meersseman (Fribourg) provided a whiff of controversy in discussing the "monastic theology" of Dom Jean Leclercq. Meersseman had a point. There were several monks among the dialecticians (for example, Anselm and Abelard); there were the canonists, too often neglected by historians in the past; there was the intuitionist or spiritual approach to theology common both to monks, such as Eadmer, and theologians, such as the Victorines. Yet in spite of this, "monastic theology" remains a significant phrase. Dom Leclercq, in the lecture that follows, met the broadside with a wave of the hand and passed on to a full and richly documented account of monastic historians and their aims. There can be no other monastic historian comparable to Dom Jean in the virtuosity with which he can produce a fresh and valuable contribution to every conference he attends. La Mendola caught him "on the hop," so to say, between Indochina and Japan, but the quality of his lecture showed no sign of fatigue. He was followed by Professor Giles Constable (Harvard), who discussed the attitude of reformers, monks, and others to the possession by monks of churches, tithes, and other *spiritualia*. Here attempts by theorists and rigorists to drive the monks off the rich field of spiritual income were at first partially successful, but the twelfth century saw a gradual return, even among the Cistercians, to the *status quo ante*. Next comes a short but remarkably taut and lucid review by Professor Duby (Aix-en-Provence) of the fluctuations between revenues in kind and in rent and the attitudes toward agrarian labor on the part of various religious orders. The Continental evidence he alleges bears out the suggestions of English economic historians that before and after the Conquest a fair proportion of the economy of monasteries rested on money. Professor Kottze (Ratisbon) looks at areas of monastic culture and suggests that while Hirsau was predominantly Augustinian in its reading, Gorze was more open to classical literature, at least in its reading habits, and

that Cluny was less interested in both Fathers and classics. If the contrast of cult versus culture is too sharp a view of Cluny and Gorze, it has at least a vestige of justification.

Finally, there are four communications. Dom Penco (Finalpia) describes the function of Fruttaria (in the foothills of the Alps north of Turin) as a kind of buffer between Italian and French, reformed and unreformed, zones. W. Kurze (German Institute of Rome), with notes on the expansion and constitution of the order of Camaldoli, pleads for a critical history of the order, and J. Plocha discusses the origin (probably Bavarian) of the Polish abbey of Mogilno. Professor Capitani (Lecce) writes at some length on the imperial connections in abbeys of central and southern Italy. On an earlier page Professor Zerbi (Milan) had given a summary of the topics discussed and distributed the appropriate bouquets of commendation.

The value and interest of the lectures are greatly enhanced by the records of the discussions that followed them and that often modified or emphasized particular points made by the lecturer. The interventions of such well-known historians as Professor Raymonde Foreville (Caen) and Professor J. F. Lemarignier were particularly helpful. One wonders whether shorthand or tape recording was the medium employed in preserving these discussions, which were presumably subedited to a certain extent. In either case the reportage is good, and the rare occasions where the English, for example, makes little sense were probably due to phonetic difficulties. The printing in general, though polyglot, is practically faultless.

As the years pass and the number of such conferences multiplies, suitable topics with the requisite depth and breadth will become harder to find until after twenty years or so another generation can take a fresh look at old issues. But the pool of research and scholarship on which the organizers can now draw is so great that we need not fear for the immediate future. (The success of such meetings, so experience seems to show, is the selection of a subject of considerable depth but limited range and the provision of an assembly of limited size and scholarly competence in that subject. Crowded conferences and scattered interests make for social, rather than academic, profit.

La Mendola deserves the support of all medievalists interested in the epoch of spiritual reform and intellectual adolescence in medieval Europe.)

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HENRY MARSH. *Dark Age Britain: Some Sources of History*. [Hamden, Conn.:] Archon Books. 1970. Pp. 221. \$7.50.

Nine literary sources of varied character and overlapping coverage supply the bulk of our historical knowledge of Britain during the half thousand years following the Roman withdrawal. At the beginning of the period the paramount event is the *adventus saxonum*. Gildas, Nennius, the *Annales Cambriae*, and *Brut y Tywysogion* deal with the struggle between the Romano-British provincials and the Germanic invaders from the point of view of the former while Bede and the *Anglo-Saxon Chronicle* reflect English traditions. In the middle of the period interest attaches chiefly to Bede's account of the establishment and progress of Christianity among the Anglo-Saxons. During the eighth and ninth centuries the central theme is political evolution culminating in the emergence of the English kingdom under Alfred. This history is traced primarily in the *Chronicle* with Asser's biography of Alfred providing additional matter at the end of the period. Two post-Conquest writers, William of Malmesbury and Geoffrey of Monmouth, also contribute some information about these earlier times, perhaps drawn from lost materials, although the latter's work is more fiction than history.

Mr. Marsh discusses each of these works in sufficient detail to permit appreciation of their individuality and comparative reliability while also revealing a pattern of relationships within the group. In this regard he is open to some criticism. He appears unaware of the dependence of much of the early *Chronicle* on Bede, and he invokes lost British sources for evidence manifestly obtained elsewhere. Even so, and despite a regrettable puerility of style (the book is crowded with "proud armies," "ruthless adventurers," "cruel ships," *ad nauseam*), the author might have produced a useful popular study had he been better informed about his subject.

As it is the book offers little to the knowledgeable student, and the inexperienced student will be ill-served by exposure to its errors.

For example, Mr. Marsh is mistaken in claiming that Britons are mentioned in early English laws (p. 44); Arthur's death is not given as 570 in the *Annales Cambriae* (p. 82; cf. p. 51); the E rescension of the *Chronicle* was maintained only until 1154, not "100 years or more after the Norman conquest" (p. 93); St. Neot, whatever his date, certainly did not live "100 years and more after Asser's death" (p. 149); the Battle of Hastings was fought in October, not September (p. 155); and Harold Godwinson was not present at the Battle of Fulford (p. 156). Moreover, the genealogy and chronology of Alfred's family is incorrect (p. 173); the discussion of Manuscript A of the *Annales Cambriae* is nonsensical (pp. 45-46); and the St. Alban entry in the *Chronicle* is not "derived from a British source" (p. 95) but from Bede (bk. 1, ch. 7), who took it from a *Passio Albani* of which Mr. Marsh is apparently ignorant (p. 112).

In addition to blunders misprints abound: for instance, interdictions for indictions (p. 93), Britain for Briton (p. 98), 751 for 731 (p. 124), Coxson for Cotton and Beohtric for Eohric (p. 145), Arwulf for Arnulf (p. 150), *Enchirdion* for *Enchiridion* (p. 151), and Aethwulf for Aethelwulf (index). The "Note on Texts and Translations," in lieu of bibliography, overlooks several recent, now standard editions and translations, notably, Williams's *Gildas*, Wade-Evans's *Nennius*, Plummer's (and now Colgrave and Mynors's) *Bede*, Whitelock's *Chronicle*, Jane's *Asser*, and Campbell's *Aethelweard*.

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Amherst

PETER CLEMOES and KATHLEEN HUGHES, editors.
England before the Conquest: Studies in Primary Sources Presented to Dorothy Whitelock.
New York: Cambridge University Press. 1971.
Pp. xvi, 418. \$23.50.

To honor Dorothy Whitelock on her seventieth birthday some of her ablest co-workers in the field of Anglo-Saxon studies have contributed twenty-two scholarly articles, which are published here along with a bibliography of

Miss Whitelock's own wide-ranging works. The twenty-two articles span the variety of disciplines that are presently being directed toward the investigation of the Anglo-Saxons: history, literature, art history, place-name studies, linguistics, numismatics, paleography, and archeology. The term "primary sources" in the subtitle embraces not only historical documents but also literary works, coins, buildings and building sites, place-names, and artifacts. Altogether these studies bear impressive testimony to the intelligence, sophistication, and scope of investigative techniques in modern Anglo-Saxon scholarship. They disclose, too, the seminal importance of Dorothy Whitelock's own contributions to Anglo-Saxon literature and history.

Compared to other collections of this sort, the present volume maintains a remarkably uniform standard of excellence. Assembled here are examples of the top drawer work of the finest scholars in the field. Peter Hunter Blair, applying shrewd critical analysis to Bede's *Ecclesiastical History*, suggests an important chronological revision in the relationships of Paulinus, Queen Aethelberg, and King Edwin that underlay the conversion of Northumbria, and in the process one learns more of Bede's own method of writing history. Bede's use of sources is further illuminated in Paul Meyvaert's subtle textual criticism of the *Libellus Responsionum* in which it is shown that Bede's text of the *Libellus* was not a contemporary forgery, as has been suggested, but a corrupt version of a much older original. J. M. Wallace-Hadrill deals expertly with relationships between St. Boniface and the Frankish Church and with antecedent Frankish ecclesiastical reform, giving us in the process a singularly persuasive reinterpretation of Charles Martel's confiscations of Church property. Kathleen Hughes summarizes and amplifies past scholarship on Anglo-Celtic ecclesiastical relationships after Whitby, tracing the work of Irish churchmen in England and English churchmen in Ireland and demonstrating their fruitful influences on one another up to the Synod of Chelsea in 816. Nicholas Brooks employs his intimate knowledge of the documentary sources to revise current opinion on the origin and diffusion of the threefold military obligation. Neil Ker, using rigorous techniques of handwriting

analysis, finds the hand of Wulfstan the homilist, bishop of Worcester and archbishop of York, in a number of Worcester manuscripts including Hemming's cartulary. Similarly, John Pope, taking issue with Dorothy Whitelock, argues from a stylistic analysis of Aelfric's rhythmical prose that the Old English version of the Ely Privilege was composed by Aelfric himself. Stylistic analysis is deftly used in Cecily Clark's study of the pre-Conquest *Anglo-Saxon Chronicle*, wherein the distinction between history and annal receives valuable clarification.

These examples demonstrate both the scholarly ingenuity and the interdisciplinary range of the contributors, and the remaining pieces follow the pattern. Henry Loyn's learned and graceful state-of-the-field chapter points to the vitality and variety of Anglo-Saxon towns and suggests promising new avenues of investigation. Political theory is represented by Dorothy Bethurum Loomis's investigation of the homilist Wulfstan's ideas of polity and *ecclesia*, which anticipate Gregorianism in their emphasis on churchmen's role in the world and the Anglo-Norman Anonymous in their rejection of papal monarchy, but differ from both—and from Carolingian political thought—in their conclusion that bishops are the proper leaders of the Christian commonwealth. The multidisciplinary approach finds further expression in Kenneth Cameron's application of place-name evidence—specifically, the Grimston hybrids—to the problem of Scandinavian settlement in the territory of the Five Boroughs, in R. I. Page's use of inscriptions to cast light on the survival of the Scandinavian language in England, and in the chapter by Olof von Feilitzen and Christopher Blunt, which catalogs the names of moneyers on the coins of King Edgar, enabling the authors to identify a considerable number of Scandinavian and Continental personal names. Michael Dolley's chapter on the nummular brooch recently discovered at Sulgrave instructs us on the author's expert methodology no less than on the subject of his inquiry. The same is true of H. M. Taylor's innovative use of "structural criticism" to date and explain the components of the Anglo-Saxon church of St. Wystan's at Repton. The technique of identifying earlier sources in Anglo-Saxon writings is demonstrated in Janet

Bately's study of classical additions to the Orosius translation and in Alistair Campbell's article on earlier heroic verse in *Beowulf*, which confirms his interpretation of the poem as a carefully constructed literary work rather than the product of an illiterate singer. The late Francis Wormald contributed a masterful treatment of the stylistic antecedents to the Winchester school of illumination back to the reign of Alfred. Several disciplines meet in Peter Clemoes's illustrated discussion of how contemporary visual representations and liturgical passages influenced Cynewulf's poem on the Ascension. René Derolez provides a useful study of Anglo-Saxon cartography and geographical knowledge in the course of analyzing the account of the Norwegian explorer Othere in the Old English translation of Orosius. And J. E. Cross uses a variety of literary and historical sources to investigate Anglo-Saxon attitudes on the "just war." The *Festschrift* concludes with an admirable article by Martin Biddle summarizing present knowledge in several fields of Anglo-Saxon archeology and urging the necessity of future interdisciplinary training such as to produce individual scholars with expertise in both history and archeology.

England Before the Conquest is not without fault. It is extraordinarily expensive, and it suffers from the organizational choppiness common to all *Festschriften*. Anglo-Saxon England awaits its new Stenton. But as a comprehensive and authoritative presentation of the current state of the art in Anglo-Saxon studies, this book will be essential to students and scholars of the period in all the relevant disciplines.

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T. B. PUGH, editor. *Glamorgan County History*. Volume 3, *The Middle Ages: The Marcher Lordships of Glamorgan and Morgannwg and Gower and Kilvey from the Norman Conquest to the Act of Union of England and Wales*. Cardiff: University of Wales Press for the Glamorgan County History Committee. 1971. Pp. xix, 704, 6 maps, 33 plates. £12.00.

This is a fascinating volume, dealing with an area and a period that offer material of deep significance: the intermingling of Norman and Anglo-Saxon with the Welsh, the contrast of

upland and lowland, the workings of Marcher lordship, the growth and decline of one of the great earldoms that played a notable part in English and Welsh politics, the impact on Marcher outlook of the Edwardian conquest of North Wales, and the varied fortunes of religion and the Church. This volume also relates the vicissitudes of economic life, the rise of the gentry, and last, but by no means least, the remarkable literary tradition of Morgannwg. With regard to this tradition, Ceri M. Lewis in a fine essay has said that, among the major historic regions of Wales, Morgannwg alone can legitimately claim to have nurtured not merely one, but two great literary traditions.

A distinguished group of writers, under the able editorship of T. B. Pugh, has produced a volume that will bear comparison with the best of the Victoria County Histories. Most of the contributors are in Welsh universities, and the volume is incidentally a tribute to modern Welsh historical writing, being specially marked by researches in depth into original sources. But the editor himself is at the University of Southampton. Other writers are in London and Leeds. Michael Altschul, who writes with much authority on the lordship of Glamorgan and Morgannwg in the thirteenth and early fourteenth centuries, is at Western Reserve University in Cleveland, Ohio.

The themes touched on above show the breadth of the treatment and its value to all students of the period. The scope might conceivably have been extended a little to deal specifically with the influence on the outlook and English policy of the lords of Glamorgan, as these were affected by Welsh power in the North under Llywelyn ap Gruffydd and by the growing power of the English monarchy, especially in the reign of Edward I. Whether, and to what extent, such developments influenced the attitude of the lords of Glamorgan toward English politics it may be impossible to say. Mr. Pugh, who writes about the fourteenth and fifteenth centuries, makes no real effort to do so. He is content, for the most part, to record the ambitions and aggrandisements of the lords, pursued as relentlessly at the royal court as in Wales. He will not even concede any qualities of statesmanship to Hugh Despenser the Younger, under Edward II, challenging such veteran authorities as Stubbs and Tout.

His summary of Warwick the Kingmaker's attitude in the fifteenth century is similarly quite uncompromising: "It was defence of his own territorial interests in the Welsh Marches that compelled Warwick to become a Yorkist, and fight against the king at St. Albans on May 22" (p. 196).

This is nevertheless a memorable volume. It sums up the quite remarkable researches of the last generation and will be an indispensable vade mecum to all future scholars in the field.

B. WILKINSON

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F. R. H. DU BOULAY and CAROLINE M. BARRON, editors. *The Reign of Richard II: Essays in Honour of May McKisack*. [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1971. Pp. xvi, 335. \$16.00.

ANTHONY GOODMAN. *The Loyal Conspiracy: The Lords Appellant under Richard II*. Coral Gables, Fla.: University of Miami Press. 1971. Pp. xi, 212. \$7.95.

The reign of Richard II has probably received more attention from English medievalists than any other save those of the Conqueror and John. The explanation lies not only in its great events but also in its contribution to the training of English undergraduates in history. First at Manchester and then at Oxford it provided a "special subject"—the study of a short period from its sources, which forms the culmination of a history student's career at an English university. This *Festschrift*, commemorating the achievements of Professor McKisack, who founded such a "special subject" at London, is a testimony to the stimulus that undergraduate teaching on the reign has given to research.

A number of useful studies examine technical aspects of Richard II's diplomacy, Henry of Derby's expeditions to Prussia, the local governments of East Anglia, London chantries, and the place of the reign in Tudor and Stuart historiography. Two describe the king's relations with the leading cities of his kingdom, though in the light of Caroline Barron's examination of those with London it is difficult to accept John H. Harvey's suggestion that Richard seriously intended moving his capital to York. Three contributors have produced excep-

tionally valuable studies. J. J. N. Palmer discusses Richard's foreign policy after 1389, showing that it made a vital contribution to the collapse of French ambitions in Italy. The Anglo-French expedition, which was planned to go there in the spring of 1397, was intended to force on the rival popes the compromise solution to which English policy had helped convert the French. R. R. Davies illuminates the last two years of the reign, describing how Richard made Cheshire the base for a remarkable concentration of power, which, benefiting from the Arundel forfeitures, extended into Wales. For the first time we obtain precise information on the scale and expense of the famous Cheshire retinue (pp. 268-69). The value of the contribution by R. L. Storey transcends the limits of the reign. He demonstrates that the order restricting the right to grant liveries to secular peers does not deserve the attention it has received from the historians of "bastard feudalism." It was an ordinance issued by king and council after, and not a statute of, the Parliament of January 1390; nor did it deal at all with retaining. At the same time his discussion contributes to parliamentary history by exhibiting friction between Lords and Commons over the maintenance of law and order.

Other studies do not merit such praise. Barbara Harvey's discussion of the monks of Westminster and Oxford may throw useful light on the relations between monastic communities and the universities, but most of it is irrelevant to Richard II's reign. In view of V. H. Galbraith's long career of remarkable scholarship, the mixture of the amateur and the professional in his "Thoughts about the Peasants' Revolt" comes as a great disappointment. His remarks on villeinage are not rooted in a thorough knowledge of the literature. It would have been so much better if he had worked out in detail his suggestion that Walsingham was indebted to the same source as the Anonimale chronicle for his account of the revolt. Does he really consider (as p. 51 implies) that they shared the same source in their accounts of the Good Parliament? The matter is too important to be dealt with in asides. The weakest study of all is J. A. Tuck's. Padded out with references to royal patronage from the sixteenth through the eighteenth centuries, it provides a thin sketch of administrative practice that hardly

accords with the promise of the title "Richard II's System of Patronage." We learn nothing of the way patronage fitted into royal policy as a whole.

The industry with which Anthony Goodman has culled the sources of the reign for his book on the Appellants is not matched by either his analytical skill or the interest of his conclusions. Roughly one-quarter of the text is a turgid survey of the events of 1387 to 1389. There follow seven chapters containing biographical studies of the Appellants and, in the case of Thomas of Woodstock and the earls of Arundel and Warwick, discussions of their landed wealth and affinities. For most of the time the connection between these and the book's main theme is not clear. Mr. Goodman may be right that Thomas of Woodstock and Mowbray needed royal patronage to maintain their position. But two pages of conclusion, containing a somewhat rhetorical question about a *crise nobiliaire* followed by guesses, are hardly a substitute for sustained argument. The book's title begs questions that are hardly raised and certainly not answered. How, for example, can it be reconciled with evidence that Richard was deposed for a few days? The state of the sources may make a more satisfying treatment difficult. But, even so, this effort is not worth a book.

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VEIKKO LITZEN. *A War of Roses and Lilies: The Theme of Succession in Sir John Fortescue's Works*. (Suomalaisen Tiedeakatemia Toimituksia, Series B, Number 173.) Helsinki: Suomalainen Tiedeakatemia. 1971. Pp. 73. 11 M.

This is a careful and detailed little study of one of the main themes in the published work of Sir John Fortescue (?1385-1473), chief justice of England, chancellor, polemicist, councilor, and political theorist. Mr. Litzen argues that the most fruitful approach to Fortescue's work is through his theories of succession. For of all his known works only one, now lost, is "irrelevant to the problems of English and French kingship." The critical problem of fifteenth-century kingship in England was that of a disputed succession, and Fortescue's explanations of what constituted the right to govern are the

heart and soul of his writing. Moreover, it is by taking this angle of approach that we can best see Fortescue as a political theorist of and in his own time.

The impetus to Fortescue's serious political writing came with Richard of York's claim to the throne in October 1460, as "lawful heir of Edward III and Henry III." To the beginning of the Lancastrian exile the following year Fortescue tried to refute York's claim, principally by denying the female right to transmit a claim to the throne. In exile he became a polemicist, writing to gain the support of the king of France for his master. Upon his re-establishment—at eighty—as councilor to Edward IV he "refuted many of his old arguments," but never abandoned those essential to the "bearing of *De Natura*," his principal work on the succession.

Mr. Litzen is concerned with exonerating Fortescue from charges of duplicity (in serving the house whose right to kingship he had for so long denied). Mr. Litzen's judgment of Fortescue recalls that of Sir John Fortescue's own great descendant, the historian of the British Army: "He was loyal in a time of treachery, upright in a time of violence, constructive in a time of destruction, and hopeful in a time of despair."

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MICHEL ROBLIN. *Le terroir de Paris aux époques gallo-romaine et franque: Peuplement et défrichement dans la Civitas des Parisii (Seine, Seine-et-Oise)*. Preface by ALBERT GRENIER. 2d rev. ed.; Paris: Éditions A. et J. Picard. 1971. Pp. xiv, 6-491. 110 fr.

Some twenty years ago Michel Roblin's *Le terroir de Paris* marked the first basic revision of the d'Arbois de Jubainville and Camille Jullian explanation for the pattern of human settlement during the Gallo-Roman and Merovingian periods in the area around Paris as well as in most of France. This second edition, essentially an amplification of the first, incorporates the research of Roblin and fellow scholars during the intervening twenty years. Like the first it is a model of scholarship combining the knowledge and methodology of the historian, geographer, geologist, archeologist, orographist, numismatist, botanist, and zoolo-

gist. Roblin's objective is to trace the origins of rural settlement, the clearing of land, the growth of villages and of hamlets, and the pattern of land and water routes. His hope has been "concrétiser l'oeuvre humaine au cours des siècles."

During the twenties when Jullian did his celebrated work on Gallo-Roman colonization he agreed with Jubainville that the names of early Frankish rural settlements or villages were derived from the names of Gallo-Roman domains whose names came from their Roman masters. Finding that numerous place names in the Merovingian and Carolingian periods ended in *acus*, he argued, for example, that the domain of *Sabinius* came to be called *Sabiniacus* from which were derived the French Savignac of the Midi and the Savigny of the north. There was a similar derivation for other such names in the Paris region as Orly, Passy, Antony, Bobigny, and Chevilly. If, then, the Merovingian and Carolingian estates and villages were the direct heirs of the Gallo-Roman, there was, according to Jullian, a continuity of civilization and agrarian exploitation running from antiquity up to the present day.

The research of Roblin appears to undermine the Jubainville-Jullian explanation by arguing with ample evidence that Gallo-Roman domains owed their names not only to their masters but also to common names. For example, the Latin *Buxus* that in French is *buisson* (bush or thicket) became *Buxiacus* and eventually Boissy or Bussy. Roblin found thirty toponyms derived from proper names as against forty derived from common names, with the origin of another ten being uncertain. He proves that many of the common names were those of streams, rivers, routes, and other parts of the physical terrain that he could not precisely date because they even antedated the Celtic or pre-Roman period. Noting also that numerous villages (communes today) around Paris were named after such saints as Etienne, Maurice, Martin, and Germain, he contends that the foundation of a church upon a certain site generally followed soon after the introduction of the cult of the saint for which the church was named. When he could date the introduction of a saint's cult into a region, he could then be reasonably accurate in his dating of the church and obtain a clearer notion of chro-

nology. Such painstaking research has permitted Roblin to date the clearance of thickets and forests in the area around Paris as well as their eventual settlement and cultivation.

A treasure house of information, this convincing and perhaps definitive work on the settlement and clearance of lands of the so-called *Civitas Parisiaca* in the Gallo-Roman and Frankish periods contains detailed studies of all the communes in the Paris area, indexes of place and common names, an exhaustive bibliography, supplementary appendixes, and fine aerial photographs.

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JOSEPH R. STRAYER. *The Albigensian Crusades*. (Crosscurrents in World History.) New York: Dial Press, 1971. Pp. 201. \$7.95.

In this readable book Professor Strayer again shows the lively confidence and ease that come from long intimacy with his favorite subject, thirteenth-century France. He begins by sketching the conditions in what, following some recent French writers, he calls Occitania during the twelfth and thirteenth centuries. He briefly details the origins and beliefs of Catharism. He then proceeds to a narrative account of the events leading up to the Crusades, the wars and politics of the Crusades themselves, the final extirpation of the heretics by the Inquisition, and the absorption of Occitania by France. There is an appendix with the author's own translations of some Catharist rituals and a somewhat exiguous bibliography.

Throughout, the author judiciously avoids catharophile extremes on the one hand and apologies for papacy or monarchy on the other. It may be that the serenity of his account somewhat mutes the human agony and that he does not, beyond the conventional expression of distaste for crusaders and inquisitors, fully plumb the pathos of the exploitation of religion by Christian leaders in order to sanction violence for political ends. Yet he conveys clearly the tragic sense of good men moving—largely unwittingly and unnecessarily—to their ruin. Pedro II of Aragon, the hero of the Catholic Reconquista, slain at Muret by the Catholic forces of Simon de Montfort; Amaury de Montfort earnestly and incompetently striving

to retain his father's conquests; Louis VIII's Occitan triumph of 1226 ending in his death from a fever contracted on the campaign; the well-meaning but indecisive Count Raymond VI hopelessly hoping to please the pope and the king and Simon de Montfort and his own people: in these and other incidents Professor Strayer gently holds an ironic mirror to the sad countenance of human folly.

For the author the chief importance of the Crusades lies in their political effects. They gave Occitania—and a Mediterranean coastline—to France, making that country the strongest in Europe and encouraging its rulers to press their power further than perhaps they ought to have done. At the same time the Crusades encouraged the papacy to be too dependent upon political and military power, particularly that of France. Thus the crisis of Crown and papacy that built under Louis IX and exploded under Philip the Fair was in large measure prepared by the Crusades.

The general reader and the student will be grateful for this book, the most coherent brief account of the Albigensian Crusades yet to appear.

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DIMITRI OBOLENSKY. *The Byzantine Commonwealth: Eastern Europe, 500–1453*. (History of Civilization.) New York: Praeger Publishers, 1971. Pp. xiv, 445. \$15.00.

This book deals with the history of Byzantium's relations with the mostly Slavic peoples of Eastern Europe and with the Byzantine heritage shared by them. The geographic focus is on the regions now constituting Yugoslavia, Bulgaria, Romania, and Russia, but the territories of present-day Czechoslovakia and Hungary also appear. The internal history of the Byzantine Empire and of the Slavic peoples as well as relations with non-Slavic (Arab, Turkish, Western) groups are discussed only to the extent that they are indispensable for an understanding of the main theme.

The geographic setting of the book is described in highly informative and readable fashion in the first chapter. It emphasizes the physical features and the medieval road nets of

the northern Balkans, of the North shore of the Black Sea, and of Russia. There follows a second chapter in which the author discusses the origins of the Slavic peoples and of some of their Turkish conquerors and their early relations with Byzantium. In the next two chapters Obolensky tells of the military, religious, cultural, and economic relations of the Byzantine Empire with the Balkan Slavs in the ninth and tenth centuries. Chapter 5 is devoted to Byzantium's relations with East Central Europe and concentrates on the missionary work of Cyril and Methodius in Moravia and on Byzantino-Magyar relations to the eleventh century. The author then turns in the sixth chapter to a consideration of the Empire's dealings with the Turks of Central Asia, the Khazars, and the Russians.

The narrative has thus reached the beginning of the eleventh century, in some cases even a later period. At this point Obolensky inserts a predominantly analytical chapter on "The Bonds of the Commonwealth," in which he describes the various types of political ties that bound the members of the Commonwealth to Byzantium—for example, the military annexation of Bulgaria by Basil II, the more tenuous subjection of Serbia, and the largely theoretical dependence of Kievan Russia. Byzantium's relations with Eastern Europe after the Fourth Crusade and the Mongol conquest of Russia are the subject of chapter 8. The three concluding chapters are again analytical and predominantly cultural. In chapter 9 Obolensky discusses the various factors facilitating or impeding the diffusion of Byzantine civilization in the Slavic world: ethnic conditions in the receiving countries; merchants, mercenaries, and monks as carriers of Byzantine civilization; geographic factors; the degree of nomadism or settled conditions; and the political organization in the Slavic countries. The tenth chapter analyzes the impact of Byzantium upon the religion and law of the Commonwealth, and the last chapter, one of the most interesting in this excellent work, literature and art. In a brief epilogue the author discusses the fate of the Byzantine heritage in the Slavic world after the Ottoman conquest of the Byzantine Empire.

The principal concept of the book, that of the Byzantine Commonwealth, is unfamiliar.

In spite of a disclaimer in the introduction one cannot help feeling that it is inspired by the model of the British Commonwealth of Nations rather than by that of the *res publica Romana* or of the Roman Empire (but see in this regard the interesting discussion of the possible survival of the Roman concept of *foederatio* on pages 276–77). Obolensky defines the Byzantine Commonwealth as "that supranational community of Christian states of which Constantinople was the centre and Eastern Europe the peripheral domain" (p. 277) and claims that "it was a real society, not a mere intellectual abstraction" (p. 3). It achieved its greatest territorial extent and unprecedented cultural and political cohesion in the early eleventh century (p. 203) and "came to a close" with the Ottoman conquest of Constantinople or somewhat later (pp. 362–63). The Byzantine Commonwealth was so real that it and its cultural centers can be mapped (see maps 7, 9, and 10). Its nature and vitality can most clearly be perceived in the realm of culture (p. 272): it created a supranational literature in Old Church Slavonic (p. 325) as well as a number of regional "recensions" of Byzantine civilization (p. 294) and permitted intellectual movements, such as Hesychasm and the thought of the school initiated by Euthymius of Trnovo, to migrate throughout its vast area. Obolensky uses the concept skillfully and discreetly, and in his hands it lends cohesion to a long work dealing with a great variety of peoples and cultures. It has its conceptual difficulties, however. Similar books could be written on Byzantium's relations with the Western and Near Eastern worlds where Byzantine civilization also had its impact. Were there additional Byzantine commonwealths in Western Europe (Venice, Southern Italy, Sicily) and in the Moslem world? Or was what Obolensky means by the term only one (largely Slavic) part of a larger whole embracing all the regions influenced at one time or another by Byzantium? Furthermore, does one do justice to the Slavic peoples by emphasizing the influence of Byzantine civilization and mentioning their pre-Byzantine or non-Byzantine traditions largely as factors facilitating or impeding Byzantinization, as Obolensky does so suggestively? Clearly the Byzantine perspective represents a healthy antidote against nationalist views of history frequently held in the

Balkan countries, but it does not tell the whole story. Whatever the answer may be to theoretical reservations of this kind there can be no doubt that Byzantine relations with, and the Byzantine impact upon, Eastern Europe are a legitimate and fruitful field for investigation and that the concept of a Byzantine Commonwealth has enabled Obolensky to produce a unified and outstanding survey of the entire field.

Indeed its strength lies in the breadth of the geographic perspective, the useful synthesis of previous investigations—many of them written in Slavic languages—and the skillful alternation between narrative and analytical chapters, which makes the book a pleasure to read. It offers a rich fare of which the summary of chapters gives only a very imperfect idea. Readers will discover in it an interesting coverage of well-known episodes in Byzantine and Slavic history, such as the account of Slavic origins, or of the ecclesiastical mission of Cyril and Methodius to Moravia and of its consequences for Slavic civilization, or of the Byzantine conquest of Bulgaria in the tenth and eleventh centuries and of the conversion of Russia. Obolensky also develops many less familiar topics—for instance, the administrative and ecclesiastical reorganization of the Balkan regions reconquered from the Slavs in the ninth century, heresies such as Bogomilism, manifestations of political or cultural resistance to Byzantinization and symptoms of regional deviations from the Byzantine model, national assemblies in Serbia, the cult of rulers and royal martyrs in Russia and Serbia, regional innovations in the liturgy and art of the Slavic countries (for example, the representations of the Feast of the Protective Veil of Our Lady in Russian icon painting). It is rare indeed that one fails to find information on topics relating to Byzantino-Slavic relations. In this regard I note the absence of a reference to recent archaeological work in Moravia, which has yielded considerable information on the Christianization of the country prior to the arrival of the Byzantine missionaries or to Basil II's subjection of the autocephalous archbishopric of Ochrida in Bulgaria to the Byzantine emperor rather than to the patriarch of Constantinople.

The schematic but adequate black-and-white maps and the ninety-three clear and carefully

selected photographs, frequently of insufficiently known art objects illustrating Byzantino-Slavic relations, are closely integrated with the text and facilitate its understanding. Footnotes are used sparingly. The book concludes with a suggestive and valuable bibliography of almost thirty pages where the reader will find, topically arranged, the basic literature both in Slavic and Western languages.

The book is so useful and interesting that it deserves a prominent role in the historical curriculum of American universities and colleges. It will of course serve admirably as reading matter for students interested in Byzantino-Slavic relations. But it is too stimulating to restrict its function. It should also find its place in courses devoting attention to the history of the Byzantine Empire, the Balkan Peninsula, or Russia, perhaps even in classes on Western medieval history.

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MODERN EUROPE

J. H. ELLIOTT. *The Old World and the New, 1492-1650*. (The Wiles Lectures given at the Queen's University, Belfast, 1969. Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1970. Pp. x, 118. Cloth \$5.95, paper \$1.95.

Most historians will agree that the discovery and settlement of the New World was one of the pivotal occurrences in modern history. But what exactly constitutes its significance? Was it the implantation of European religion, language, and culture upon a new continent, or was it the exploitation and domination of the Amerindian civilizations with European technology and institutions? The question has another side, too, and it is this to which Professor J. H. Elliott addresses himself. What was the impact—intellectual, sociological, economic, and political—of the discovery of America upon Europe?

The question is more easily formulated than answered, and it is unlikely that anyone will give a more succinct, thought-provoking, and meaningful analysis of it than Professor Elliott has done. He grapples with the puzzling problem of why the incorporation of the New

World into the intellectual pattern of the Old took so long. The main impediments, as he sees them, were the people's inability to fully comprehend the new societies. This was due in part to the barriers of time, distance, sociointellectual heritage, and language. Since the process of comprehending something new and different requires the reassessment and possible abandoning of many traditional assumptions, it was difficult for European society to come to terms with America. Elliott brilliantly summarizes the process by which the Indies were gradually assimilated into the European system of thought.

He then evaluates the impact of New World silver upon the economic life of Europe and the changes in political direction brought about by the expanded area of political jurisdiction and conflict. While rejecting the notion that the Price Revolution was caused by the influx of American treasure, the author sensibly demonstrates that its presence helped keep the inflation going once it was started. Similarly, he shows that although European politics certainly was not dominated by the presence of America, by the middle of the seventeenth century America was no longer a mere appendage of the Castilian and Portuguese Crowns but had been integrated into the political-diplomatic structure of Europe.

This is a thoughtful and well-written book, which should stimulate and point the way for future study and research.

DE LAMAR JENSEN

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STILLMAN DRAKE. *Galileo Studies: Personality, Tradition, and Revolution*. Ann Arbor: University of Michigan Press. 1970. Pp. 289. \$8.50.

We have all met Galileo. General education and civilization courses, basic surveys or cultural courses in the sciences, and of course the history of science itself have introduced us to him so often that we greet him familiarly by his first name—a symbolic judgment given to no other individual in the history of modern science. But few have come to know him as well as Stillman Drake. A lifetime of scholarship devoted to the great Tuscan iconoclast has brought to light essential manuscripts and pro-

duced critical editions of key works. In addition, translations and commentaries of and upon Galileo, including the work of his as well as of our contemporaries, have created a corpus Galileanna so closely connected with Professor Drake's name that we have come to think of this distinguished scholar as a proper familiar, a timeless companion of his own hero (a Sagredo come to life!).

In these essays, whose title evokes Alexandre Koyré's brilliant pioneer *Etudes Galiléennes* (1939), Drake pays homage to a great historian and clearly sets out "to suggest the complementary character" (p. 14) of his own internalist and psychological method to Koyré's externalist and philosophical approach. Thus methodology sets the stage, and where Drake's concern with *Personality, Tradition, and Revolution* (the subtitle and true theme of his work) gives us a sketch—penciled, fuzzy at the edges, drawn to scale—in short, a sketch of a human being, Koyré's logical analysis has given us a Renaissance oil, an intellectual portrait beyond mutable experience. In our asymptotic search for truth we are indebted to both.

Of the thirteen papers assembled in this volume all but three have been published before, between 1957 and 1970, mostly in *Isis*, *Physis*, and *Osiris*, but also in the *British Journal for the History of Science*, the *Journal of the History of Ideas*, and in special volumes such as the *Saggi su Galileo Galilei* (Florence, 1967) and *Art, Science and History in the Renaissance* (Baltimore, 1968). Their re-issue in this volume has allowed Drake to recreate them, fusing, altering, and redrawing the scattered arguments of his earlier gleanings into substantial, independent essays of value to both general and specialized readers. Of the new pieces, "Physics and Tradition before Galileo" and "The Scientific Personality of Galileo" will be of most interest to the general reader. The former supplies a much needed narrative statement on the filiation and continuity between Galileo's ideas and sixteenth-century physical thought. The latter teaches us all, as taught we must be time and time again, that thoughtful men as well as the blind conglomerate pressures of circumstance make history, that the personality of Galileo was a significant element in his leadership of the nascent scientific community of Renaissance Europe, that a maturing scientific

conscience would become a force to contend with in human affairs.

Taken together, these essays sing with the joy of scholarship, yet what we have are mainly tone poems and figured pieces; excellent though they may be, we can only hope that larger synthesizing compositions lie ahead.

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CARSTEN HOLBRAAD. *The Concert of Europe: A Study in German and British International Theory, 1815-1914*. New York: Barnes and Noble, 1971. Pp. x, 234. \$8.00.

Carsten Holbraad offers a basically well-documented study of the evolution of German and British attitudes toward the European states system in the nineteenth century, a topic of major importance to diplomatic historians increasingly concerned with changing assumptions about the nature and framework of international relations. From a historical standpoint, however, both the author's substantive presentation and his interpretation of the developments in international relations theory are undermined by methodological limitations, stemming most explicitly from his overly schematic organization and diagrammatic analysis.

The attempt to distinguish rigidly between two procedural stages—first, “collecting, analyzing, and arranging” his data and, second, logical and historical analysis—involves the author in methodological problems from the outset. The initial stage is carried out by the presentation of capsule summaries of successive individuals' ideas about the Concert of Europe, with the first half of the book for Germany (including Austria for the first part of the century) and the second half for Britain. Under each country the strands are separated rigidly into “conservative,” “balance of power,” and “progressive (i.e. federative)” categories, with additional separate sections devoted to the critics of each strand.

These basically successful summaries provide the book with a certain substantive contribution. However, the strict, catalog-type categorization proves too artificial and often prevents the exploration of the complexities and changes in attitudes of given individuals or in the relationship of the ideas to their historical

environment. For instance, Gentz and Metternich fit neatly into the German “conservative” category, which is in fact formulated around their dominant view of the European Restoration, but the same category seems a less appropriate label for the subsections covering their German critics. And the “conservative” strand—both pro and con—proves less distinguishable in England, where poor Castlereagh is left somewhat stranded between categories.

When Holbraad virtually defines the nineteenth-century “balance of power” category in terms of Ranke's thought, he raises interpretive questions he does not really answer. And except in terms of his basic assumption that “ideas themselves are historical factors” (p. 113) he fails to justify his inclusion of the professors and pamphleteers who rub shoulders with diplomats and men of state in his summaries. Thus more serious conceptual difficulties arise on the author's avowed “second stage” where logical and historical analysis are his aim. Had relatively more space been devoted to this process and less to the catalog itself, many of the author's perceptive insights might have been more cogently developed. As it is there is too little concern for the political milieu of the abstracted pronouncements, so that the different strands of thought become not the historical streams the author claims but a series of cataloged ideas related to each other only in facile logical constructs and neat diagrams (pp. 106, 135, 203) rather than in terms of developing historical situations.

This problem makes it difficult for Holbraad to substantiate the underlying themes that emerge from his implicit search for the theoretical roots of the disasters that beset twentieth-century European international relations. Holbraad sees a polarity developing by the First World War between the “organizational-progressive” British attitudes that were to encourage twentieth-century federative efforts and the “imperialistic anti-European” German attitudes that were to thwart such hopes and subvert the European continent. Such themes are provocative, but few will find either convincing or clear, for instance, the charted interrelationships through which Ranke's “balance of power” ideas—“the moderate and noble ideas about European politics”—became subverted by Droysen's “nationalist criticism” and He-

gel's "interstate anarchism," through the medium of Treitschke and Bismarck, so that they end up in Germany "degenerated into selfish and brutal doctrines of anti-European policy." Yet the whole course of this complicated development Holbraad is able to summarize in a brief diagram (pp. 106-07).

At a time when many historians are sympathetically looking to social-science methodology for meaningful analytic frameworks and explanatory generalizations Holbraad's book appears more as a warning about the difficulties of such approaches than incentive to use them.

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M. I. MIKHAILOV. *Istoriia Soiuz Kommunistov* [The History of the Union of Communists]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 561.

This is M. I. Mikhailov's latest publication in a series of articles and monographs pertaining to the lives, activities, and teaching of Marx and Engels. As in Mikhailov's earlier works, especially in his *Souiz Kommunistov—Pervaia mezhdunarodnaia organizaciia proletariata* (Moscow, 1960), in this volume the author also discusses, but in greater detail, revolutionary activities and societies in Western Europe from the 1830s to 1852. The book encompasses events preceding the formation of the Communist League in 1847 and its activities up to the dissolution of the union in 1852.

M. I. Mikhailov concentrates on the contributions by Marx and Engels in the formulation of the theory of scientific communism and their endeavors to apply it in practice. He emphasizes that Marxian scientific communism is a unity of theory and practice, and he criticizes Western Marxist writers, such as Franz Mehring and Gustav Mayer, for having failed to recognize this unity.

Organized chronologically, the book consists of three parts. The first part deals with the nature and developments of German emigré societies in Switzerland, Belgium, France, and Britain. It describes the emergence and the activities of the German People's Union, the League of the Rejected, Young Germany, and the League of the Just. The author dwells on

views and activities of their prominent members and devotes a chapter to the discussion of Wilhelm Weitling's views. The second part deals with the League of Communists during the revolution of 1848 and its role and activities in Brussels, Paris, and in German cities, especially in Cologne. The third part relates the activities of the League of Communists after the revolution and its dissolution after the trial of communists in Cologne in 1852.

Written strictly from the viewpoint of Marxism-Leninism, with inevitably abundant references to Marx and Lenin, the monograph is notable for its detailed account of complex and diverse revolutionary trends in West European countries, of the natures and activities of different revolutionary societies, and of important individuals involved in them. The book is heavily documented with both primary and secondary sources. The concluding chapter, "Sources and Historiography," summarizes and gives the author's comments on many works from the 1830s to the present dealing with his subject matter. Needless to say, Marxist historians are viewed positively, while non-Marxian Western scholars or those with differing views are not treated with much tolerance. The works of East German communist writers are especially praised. Such one-sidedness detracts from the value of this comprehensive and thorough work.

The voluminous bibliography attests to the painstaking labor of the author. To the student of Marxism, and especially to those interested in the period covered, the book is valuable for reference and as an example of Soviet thought, methods of research, and evaluation.

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JOHANNES WENZEL. *Jakob Burckhardt in der Krise seiner Zeit*. Berlin: VEB Deutscher Verlag der Wissenschaften. 1967. Pp. 176.

E. M. JANSSEN. *Jacob Burckhardt und die Renaissance*. (Jacob Burckhardt Studien, Number 1. Speculum Historiale: Studien op Geschiedtheoretisch, Geschiedfilosofisch en Ideënhistorisch Gebied, Number 5.) Assen: Van Gorcum & Comp. N.V. 1970. Pp. 260. 35 gls.

Jacob Burckhardt's historical writing subtly reflects its author's attitude toward the course

that Europe was taking in his lifetime. This subjective quality provides much common ground for two books representing opposing ideological spheres.

Wenzel's scholarly, yet polemic monograph, the first comprehensive East German Marxist treatment of Burckhardt's politics, contends correctly that Basel's aloof, highly original historian was really involved if not publicly active in the crisis of the nineteenth century. Burckhardt's reaction, Wenzel argues, was not *apolitique* but ideological opposition to democracy's threat to traditional Swiss patriarchal society. His efforts to transcend the crisis through asceticism and culture, as well as his pessimism concerning progress, were rooted in a mature, "reactionary" world view that recognized social evils while claiming that nothing could be done about them.

This thesis draws considerable support from Burckhardt's apparent lack of social conscience, but otherwise it limps. From either a Marxist or a liberal viewpoint Wenzel's conception of the nineteenth-century crisis is simplistic. He fails to clarify the key concept, democracy, which he applies indiscriminately to majority rule, socialism, the rise of the masses, and even diverse revolutionary movements condemned by Karl Marx. Wenzel neglects to deal with a fascinating coincidence of viewpoint in Burckhardt's and Marx's critique of the revolution of 1848, the subsequent conservative reaction, and other symptoms of social unrest. Moreover, he exaggerates the sociopolitical reasons for Burckhardt's attitude, which were important but not predominant. Werner Kaegi's monumental biography presents a more credible picture of Burckhardt, concerned mainly with teaching and other duties, engrossed in the beauty of art and nature and in the wisdom of history. Materialistic preconceptions have perhaps led Wenzel to misread the Goethean humanism in Burckhardt's life: it is likely that he pursued the Ideal more as the ultimate goal of existence than as an escape from social strife.

Its shortcomings notwithstanding, the book has scholarly merit. It reflects careful, extensive reading in the primary and secondary published sources, listed in a lengthy bibliography. It is also well written. From his unusual perspective the author sheds new, if harsh light

on previously neglected facets of Burckhardt's political thought.

Where Wenzel's study is doctrinaire and one sided, Janssen's is free from bias and open to many points of view. Nevertheless, their ideas of Burckhardt in the crisis of the nineteenth century are surprisingly similar; a well-informed, reactionary conservative, committed to vigorous, intellectual resistance to the principal historical forces of his time. But, whereas the Marxist would have Burckhardt fighting for a patriarchal social order, the Dutch scholar has him living in the past, devoted to an idealized conception of the Italian Renaissance. They both underestimate the strength and consistency of his secularized Christian humanist convictions.

The first in a projected series of studies on Burckhardt's historical works, Janssen's book is intended to offer a thorough analysis of "the structure and conceptual organization" of the classic essay on the Renaissance. The text treats the more important themes roughly in their original order. A separate section, fully two-thirds of the book, contains detailed, often highly perceptive notes on a wide variety of related topics. This method, however, neglects the essay's structure as a whole. Hence Janssen's masterful analysis has a curiously unfinished quality.

Designed to complement volume 3 of Kaegi's biography of Burckhardt, Janssen's study is unconvincing at the few points where it implicitly contradicts Kaegi. Janssen fails to support, for instance, his contention that Burckhardt is a "verkehrter Pygmalion," a historicist who prefers to love a dead, idealized image of the Renaissance rather than embrace the living reality of his own time. This widely held opinion has been substantially corrected by Kaegi's stress on Burckhardt's basic good sense, as well as by Peter Gay's emphasis on Burckhardt's clear insight into the essential ambivalence of the Renaissance's relationship to modern civilization.

A product of rich erudition and sound craftsmanship, Janssen's study reflects meticulous research into Burckhardt's published writing, careful reading of Kaegi's biography, and thorough familiarity with secondary literature up to 1967 except for Gay's contribution to the *Festschrift* for Hajo Holborn.

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ARNO J. MAYER. *Dynamics of Counterrevolution in Europe, 1870-1956: An Analytic Framework*. New York: Harper and Row. 1971. Pp. 173. \$7.00.

This book is in two respects similar to Professor Mayer's earlier works. He writes it, he says, as "a confirmed leftist critic of those Allied and American policies, both foreign and domestic, that condoned or advanced, intentionally or unintentionally, the counter-revolutionary side in the era of the communist revolution." He writes it also as a man who is aware of the danger of forcing the data to fit the construct, but who fails to guard against that danger. In one respect, however, it is different from his other writings. They are so overstuffed with data that their conclusions, though preformed, appear to result from letting the card indexes speak for themselves. Here the opposite form of self-deception is relied on: he provides no data at all, and the central thesis is no more than an assertion of political belief.

What is this central thesis? It is not that "revolution is more productive of human growth, betterment and dignity than counter-revolution," though this is one of Professor Mayer's convictions. It is not even that, in the age of the communist revolution, only communist revolution can truly be revolution. It is that in such an age of crisis politics all disagreement with or opposition to revolution must by definition be counterrevolution. Counterrevolution is not merely opposition to revolution; it is antirevolutionary. Counterrevolution is the product of the constellation of world history and not of localized national aberrations. Whether they have been aware of it or not, "those Allied and American policies that condoned or advanced . . . the counter-revolutionary side" have inevitably developed a striking family resemblance to the policies, "both foreign and domestic," of the Nazis.

This argument is not impressive as a statement of political belief. Instead of castigating those establishments which, on his own showing, have had no choice but to become counter-revolutionary, Professor Mayer should either be organizing their liquidation, or be striving to bring to an end the age of crisis politics that has produced them. As an aid to historical understanding the argument is, I believe, worth-

less because it is itself a typical product of that age.

Professor Mayer is to some extent aware of this. He closes the area of crisis politics in 1956 and refrains from extending his "heuristic construct" to the problems of the Third World: so to extend it would, he says, give it "a blunted cutting edge." In fact, it would blow it sky-high. At the other extreme, in a tailpiece entitled "Internal Causes and Purposes of War in Europe, 1870-1956," he urges that the wars of this era were peculiarly the outcome of strained and unstable internal conditions: but he has to admit that his case rests on an argument that Aristotle, Bottero, Montaigne, and Bodin discussed in other ages. In this case, moreover, he has also to admit that the cutting edge cuts both ways. Unstable internal conditions disposed elites to take exceptionally drastic pre-emptive solutions. But they also disposed them to avoid recourse to war—to embrace appeasement. In the neutral vocabulary of the social sciences, which Professor Mayer has adopted in order to keep passion in control, his conclusion is that "the calculus of the internal political effects of . . . war is more likely either to deter or to encourage recourse to war in a revolutionary era and under conditions of internal instability than in times of domestic and international equipoise."

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HARALD VON RIEKHOFF. *German-Polish Relations, 1918-1933*. Baltimore: Johns Hopkins Press. 1971. Pp. xii, 421. \$15.00.

Professor von Riekhoff's scholarly monograph is testimony to the fact that good will or even a recognition of mutual need to stand together against common dangers are not the predominant qualities of statesmen, especially when dealing with problems as complex and emotionally charged as those affecting relations between Germany and Poland from 1918 to 1933. With its corridor cutting through German territory and its inclusion of lands long under German dominion, the new Poland was for Germany nothing less than a national affront, a constant reminder of a lost war and a spur to

revise or abrogate altogether the humiliating conditions imposed on Germany after that war. "Even if the German government had correctly evaluated the potential of Poland as a partner and been willing to sacrifice its revisionist claims on the altar of reconciliation," Professor von Riekhoff says, "it remains open to serious question whether German popular opinion under the democratic process could have been induced to accept a solution of this nature" (p. 385).

It should have been easier and it would certainly have been politic for the Poles to seek good relations with Germany after 1918. They, after all, had "won" as a result of the First World War, and with the menace of Bolshevik Russia on their flank they might have seen their national interests best served in making every effort to reconcile Germany to the new political situation in Europe. Yet the Polish government, too, was swept along by popular fears and resentments in carrying out harsh and often vindictive measures against the German minority in Poland, and, fearing that any concessions to German revisionists would be a prelude to a fourth partition of Poland, the Polish government steadfastly opposed a readjustment of German territorial claims.

Professor von Riekhoff concludes that the history of Germany's relations with Poland during the Weimar period is a study in failure in political conception and practical application. The only redeeming grace he finds in German policy was "the firm commitment on the part of the German government to seek a revisionist solution by peaceful means," whereby it succeeded in preserving peace, however precariously, during the Weimar era (p. 386).

In the light of Professor von Riekhoff's depressing study it would seem that German-Polish tensions would never have been resolved until either or both sides had been exhausted in the conflict or until both were brought under the dominion of a superpower, as is now the case. It is a big price to pay for national pride and bigotry.

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STANISŁAW NAWROCKI. *Policja hitlerowska w tzw. Kraju Warty w latach 1939-1945* [The Nazi

Police in the So-Called Wartheland, 1939-1945]. (Badania nad Okupacją Niemiecką w Polsce, Number 10.) Poznań: Instytut Zachodni. 1970. Pp. 297. Zł. 50.

JAN SZILING. *Polityka okupanta hitlerowskiego wobec Kościoła Katolickiego, 1939-1945: Tzw. Okręgi Rzeszy: Gdańsk-Prusy Zachodnie, Kraj Warty i Regencja Katowicka* [The Nazi Occupant's Policy toward the Catholic Church, 1939-1945: The So-Called *Reichsgauen*: Gdańsk-West Prussia, Warta-Land, and Katowice Regency]. (Badania nad Okupacją Niemiecką w Polsce, Number 11.) Poznań: Instytut Zachodni. 1970. Pp. 306. Zł. 60.

These two books represent volumes 10 and 11 in the series Studies on the German Occupation of Poland, which began in 1946. The first monograph deals with the role of the German police in one of the Polish provinces incorporated into the German Reich, while the other discusses Nazi policy toward the Roman Catholic Church in all Polish provinces that in the aftermath of Poland's defeat in 1939 became an integral part of the Reich.

Both books could be described basically as typical doctoral dissertations not unlike many of the Ph.D. theses written at American universities. They are loaded with lengthy and detailed footnotes; the research appears meticulous and based on original German sources; the final outcome not exactly earthshaking but not uninteresting either. In the latter respect the volume analyzing Church policy is by far more interesting than its companion volume, which adds little to our knowledge of the German rule of terror in Poland.

Since both monographs deal with rather narrow problems of interest to only a small group of historians, I shall limit myself to a few comments of a more general nature. To begin with, both books are refreshingly free of any ideological connotations and devoid of the previously obligatory and frequent references to the "classics of Marxism-Leninism" that used to be cited as ultimate authorities regardless of the subject. Second, one cannot help noticing for the umpteenth time the German thoroughness and determination to put everything down on paper with the result that the history of Nazi Germany is rapidly becoming the best-documented history of all times. Last, the volume dealing with Church policy appears to

confirm once again the highly ambiguous role of the Catholic Church vis-à-vis Nazi Germany. To be sure, about one-third of all the Polish priests in the incorporated provinces perished at the hands of the Germans, yet a large number of those who survived managed without undue difficulty to accept the new reality and to collaborate with their German counterparts who in their turn acted as obedient tools of the Nazi regime in an effort to eradicate everything that was Polish. The rationale in both cases seemed to be the preservation of the Catholic Church at almost any price.

This policy of relative accommodation reappeared with equal strength in the course of Communist takeover of Poland shortly thereafter, and, once again, the Church succeeded by and large in arriving at a *modus vivendi* with the new rulers in the name of preserving the Church's power and influence.

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ERIC ASHBY and MARY ANDERSON. *The Rise of the Student Estate in Britain*. Cambridge, Mass.: Harvard University Press. 1970. Pp. x, 186. \$7.00.

Though the influence of universities upon students is a familiar enough theme in the history of higher education, the influence of students upon universities is little known. In tracing the rise of the student estate in modern Britain the authors depict the ways in which students have exercised their right of membership, inherent in their role as scholars, in the ancient and modern universities of Britain. Greater attention was paid to the student estate in Scotland than in England until the twentieth-century development of the provincial universities: though student unrest, albeit rare, was not unknown in nineteenth-century Oxford and Cambridge, their students possessed no rights paralleling their Scottish counterparts' right of choosing a rector, though this practice had been heavily qualified save at Glasgow. Parliamentary recognition of this franchise was conferred in 1858, and thereafter students in the four universities of Scotland were represented as well—in more than token fashion—on the university courts. Yet little was made of this opportunity by the students' elected representatives.

Of greater importance was the concurrent development in Scotland of the Students Representative Councils, which in 1889 Parliament recognized as embodying the student estate; their most effective work lay in the suggestions addressed to the Scottish academic senates concerning the teaching and discipline of the university. The gradual infiltration of such ideas south of the Tweed culminated in 1921 with the establishment of the National Union of Students, which until the late 1960s contented itself with a moderate approach to the problems raised by its members. Occasionally voices have been raised in criticism of such a vocational approach: "Direct work among the progressive forces in society . . . should be a fundamental principle for all members of the university." Those words, surprisingly, date to 1943, but the sentiment certainly echoes in our own time. Indeed, the authors conclude their historical survey with the unresolved question whether the student estate in Britain deserves representation on university councils and senates while it functions, at the same time, along trade-union lines to protect its interest against other (and possibly conflicting) interests within the university. The student estate, they argue, cannot expect the privileges of both.

With their interest in the present role of the student estate—Sir Eric Ashby was vice-chancellor of the University of Cambridge from 1967 to 1969—they supplement their historical study with what may be characterized as a tract for the times. In a final chapter, which merits inclusion in any university administrator's handbook, the authors outline a taxonomy of the "student conscience." Sympathetic to reasoned dissent, they articulate as well a strategy of "aggressive tolerance" for those who would disrupt in order to destroy. Not everyone, however, will share the authors' implicit assurance that university administrators will be able always to discriminate between legitimate protest and "campaigns of disruption." Yet in calling faculty and administrators to reflect upon the values that it is their unique lot to protect and forward Sir Eric Ashby and Miss Anderson have effectively utilized history to gain a perspective upon the troubled present of the university.

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A. R. B. HALDANE. *Three Centuries of Scottish Posts: An Historical Survey to 1836*. Edinburgh: University Press; distrib. by Aldine Publishing Company, Chicago. 1971. Pp. xiv, 336. \$9.75.

Mr. Haldane's volume is a welcome addition to books on the British post office. It carries the record down to 1836 when Sir Francis Freeling ended nearly forty years of service as secretary and virtual controller at the headquarters in London. Mr. Haldane has long been interested in the use of the roads in Scotland; he is a member of the legal profession (a writer to the Signet) with his offices in Edinburgh.

Postal services in Scotland began long before James VI became James I of England on the death of Queen Elizabeth in 1603. About this time the posting arrangements for mail carriage were opened to the public, and the headquarters for Scotland as well as for England were centered in London. The narrative is well organized and attractively written throughout. The chapters in the latter part of the volume following the introduction of the mail-coach services are of special interest as they are based on the abundant material available at the London headquarters but not previously used by historians. There are chapters on the town and city posts and on the country posts, with a fascinating chapter on the work of the village postmasters. There is a chapter on the posts to the Isles, another on budgetary problems, and a diverting account of "Abuses and Evasions of the Post."

Advance had been slow in the eighteenth century; there were 34 post towns at the beginning and 164 by 1800. The quarter century of war against the French Revolution and Napoleon (to 1815) had led to a much greater demand for news. Penny posts were being attached to post towns to give mail service to neighboring villages. The change was dramatic. Mail coaches were reaching speeds of nine miles an hour on the main roads, and the penny post was extending rapidly. By the time Francis Freeling died in 1836, Scotland had 250 town posts, 80 of them with penny posts. The evidence of this surprising change in the postal picture is shown in a large folding map of 1837, to be found in the pocket in the back cover. And in 1840 something like a revolution had come with the extension of the penny rate to letters posted throughout the British Isles.

This volume is well supplied with illustra-

tions, some reprinted documents, a bibliography, and numerous footnotes. The press of the University of Edinburgh has produced an attractive book that is worthy of its contents.

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LACEY BALDWIN SMITH. *Henry VIII: The Mask of Royalty*. Boston: Houghton Mifflin Company. 1971. Pp. xii, 335. \$8.95.

Lacey Baldwin Smith in this study of Henry VIII provides not a biography but "a portrait of a mind in operation." The portrait is deduced from the king's actions; he left no personal reflections on his life and, indeed, it is argued here, was incapable of original thought. Yet this is not merely a subjective view of Henry VIII: it is based on documentary evidence that in most cases convincingly supports the interpretation. The book is above all a study of the exercise of power and its effect on a highly emotional, insecure man, bound for personal and political reasons to maintain the myth of divine-right monarchy.

The need to preserve his independence dominated the king in the 1540s, the years under scrutiny here. Already the rival factions were forming, beginning the struggle for power that would erupt after his death. Henry VIII asserted his authority by manipulating people, deliberately creating fear and confusion in those nearest to him in order to discover their hidden purposes. Information was the basic instrument of coercion, and he collected it assiduously not only by this means but by the routine sifting and absorbing of the flow of detail put before him by his advisers. This knowledge, retained by an exceptionally good memory, gave Henry the professional competence in government that helped him to keep control to the very end. The legend of an idle king is here refuted. His well-attested reluctance to read and sign documents until he returned from hunting—when fatigue often became the next excuse—is seen as evidence not of slackness but of a fundamental difficulty in decision making. When the duty could no longer be postponed Henry applied to it a "structured and ritualistic" approach that evaded the responsibility of reaching a decision based on real understanding. This approach appeared most clearly in religious matters. To Henry

VIII, as to many of his subjects, religion was essentially a bargain with God, both temporal happiness and eternal salvation being the reward of a scrupulous observance of God's laws. When disaster struck, the failure that had invoked the divine vengeance had to be located and removed. In Henry's case this necessity eventually drove him, fearful though he was of committing himself, to put all his trust in his own judgment. As supreme head of the Church Henry stood alone, sustained only by his high conception of the role of a Christian prince.

War was a relief from tension, an absorbing activity calling for no complex decisions. Its prime purpose was psychological: to enable the participants to achieve honor and glory. Beyond this it was held to improve the moral fiber of the nation and might indeed for a time reduce political and social discord by exporting troublemakers of all ranks. To condemn Henry's wars for their futility and waste is to judge them by modern standards—but perhaps also, one might add, by the standards of the ordinary people. The "despotism of the mind" exercised by Henry VIII over his kingdom was in essence the despotism of a king who reflected the thought patterns of the ruling classes. The new light shed on Henry VIII's personality in this compulsively readable study illuminates also the courtly society in which he lived.

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THOMAS ROGERS FORBES. *Chronicle from Aldgate: Life and Death in Shakespeare's London*. New Haven: Yale University Press. 1971. Pp. xx, 251. \$10.00.

Mr. Forbes, professor of anatomy and teacher of the history of medicine in the Yale University School of Medicine, has, with the support of two grants from the Public Health Service, produced a book very narrowly based on four volumes of parish registers and eight volumes of parish clerks' memoranda books of the parish of St. Botolph without Aldgate, London, for the years 1558–1625; a book in which he gives data, sufficient for an article, on baptisms and burials, stillbirths, illegitimacy, age at death, causes of death by disease, and acciden-

tal and violent deaths. These straightforward demographic data, the essential part of the book, are apparently carefully compiled (in spite of a slight difficulty in transcribing accurately) and are sensibly handled; and because of the extraordinary detail the parish records give over some spans of years (cause of death and age at death, for example) the data are of unusual interest. The rest of the book is chaff. Forbes takes an antiquarian delight in collecting notices of births, sicknesses, accidents, and deaths, which he quotes frequently and at length; quotations constitute about a quarter of the book. Although he has tried to get up the age of Will Shakespeare from W. E. Lunt's *History of England* and A. L. Rowse's *England of Elizabeth* and from more specialized works that are sometimes not those most appropriate to Forbes's needs, the author's attempts to place his parochial findings in a broader social and demographic context and to relate them to public policy fail. The reader is not helped by being told that a "swaddler" in 1600 was "perhaps a Methodist preacher," that a schoolmaster at this time was "also known as an informer," that "piracy was not unusual as late as the sixteenth century," that William Sebright, having subscribed a mayoral precept to the wards in 1594, was lord mayor (as town clerk from 1574 to 1613 he regularly subscribed such precepts), or, at one point, that the parish of St. Botolph without Aldgate was "outside the city." The author generalizes from D. V. Glass's introduction to *London's Inhabitants Within the Walls, 1695* to establish the relative poverty of the parish in the sixteenth century, when he might have put contemporary subsidy assessments to this and other uses. He has apparently made no use of the London Corporation records, which would have shed light on the city's concern with orphans, the poor, the sick, and other matters that he treats with crude sketchiness and unnecessary speculation. It is a pity that Forbes did not publish the demographic kernel of his book in an article, which would, I believe, have introduced to a wider, interested readership these remarkably full parish records and enabled more scholars to relate his data for a London parish to other recent local population studies.

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S. G. CHECKLAND. *The Gladstones: A Family Biography, 1764-1851*. New York: Cambridge University Press. 1971. Pp. xvi, 448. \$16.00.

This is a very rich book, really several books in one. It is, first of all, the history of a family—the story of how John Gladstones, who came to Liverpool from Leith in 1787 at the age of twenty-two in order to set up as a grain merchant (and who dropped the terminal “s” from his name during the same year for reasons of business convenience), raised with his second wife, Anne Robertson, a family of four sons and two daughters. As such, the book is a study in character and personality—not merely of the young William Ewart individually, but of his nurture within the family circle where his moral and intellectual outlook was shaped.

Second, the book is the history of the accumulation of a fortune. When John Gladstone started as a corn trader in Liverpool he invested £1,500 in a partnership and contracted to receive £80 a year as annual salary. When he died, in 1851, he was worth something like £750,000. Shipping and West Indian plantations played a major part in the growth of Gladstone's wealth; and Professor Checkland, a distinguished economic historian, has gone to immense and painstaking lengths to trace the history of that growth from the available business and family records. If only as a detailed study of how merchant (as distinct from industrial) capital could be built up in the early nineteenth century, this book will prove invaluable.

Urban history, social and political, is the third major theme. John Gladstone became one of the leading figures in the great trading port of Liverpool, and, as such, was closely associated with George Canning and William Huskisson. Professor Checkland gives one a vivid sense of how local rather than national issues, primarily economic, went into the making of the elder Gladstone's political stance and thus, to some extent, into the making of that of his more famous youngest son. That stance was decidedly conservative, though John Gladstone had begun his Liverpool career as an ally of the local Whig-Radicals. This book is essential reading for anyone interested in the question of how and why the greatest Liberal statesman of the nineteenth century came to be hailed at the start of his political life by Macaulay as the

rising hope of “those stern and unbending Tories who follow, reluctantly and mutinously, a leader whose experience and eloquence are indispensable to them, but whose cautious temper and moderate opinions they abhor.”

Religion constitutes the fourth strand of this family biography. John Gladstone ceased to attend the Scots Kirk and, along with his second wife, became one of the chief pillars of Liverpool Evangelicalism. How could he reconcile his Evangelicalism with his ever larger slaveholding interests in the West Indies? It was this issue that involved him in a struggle with another great Liverpool family, the Croppers, who were Quaker Abolitionists, but whose own fortune came, in part, from American slave-grown cotton. It was this same issue that increased the psychological tensions within the Gladstone family. For Mrs. Gladstone was unable to reconcile herself to her husband's role as an Evangelical slaveholder.

Her health was frail, in any event. And illness is yet another theme that pervades this book. John Gladstone's first wife had died young. Anne, his eldest daughter, the “saint” of the family, died after protracted suffering at the age of twenty-seven. Her younger sister Helen was a perpetual invalid, sporadically addicted to laudanum. Spas, medicines, treatments, and physicians are constant factors in the history of the Gladstone family. So much so that, at times, one is under the illusion that one is reading a harrowing “medical” novel. The names of some of the characters, such as “Dr. Fallati,” Helen's doctor in Baden-Baden, and “Sporn,” her maid, contribute to that illusion.

Professor Checkland treats William Ewart's brothers—Tom, barrister and M.P.; Robertson, businessman and mayor of Liverpool; and John Neilson, naval officer and country gentleman—with loving attention. But it is the glimpses of the “grand young man” of the family that grip one most strongly: a poem written by him at the age of eight, after a passer-by had saved his life from the threats of an armed madman: “Oh! How would my parents grieve/ If they heard that I was dead/ Oh! How would my parents grieve/ If the black Worms rotted even my head”; a letter to his father, thirteen years later, giving thanks to God for having been beaten up in his rooms at Christ Church

by a party of young men who "were living in sin and had rejected Christ as their Savior"; or the author of *The State in its Relations with the Church* (1838) flinging his hat into the air and shouting "Long Live Liberty!" as he went horseback-riding out of the Papal States in 1839.

To have carried off the ambitious scheme of this book with total success would have required a historian of genius and would have resulted in a classic work. The book falls short of that. The style is, at times, curiously crabbed; the long supporting cast is not always sketched as vividly as it might have been; a piling up of details occasionally threatens the total structure. But, when all is said and done, there is no doubt that this is a major achievement, indispensable for an understanding of the Gladstones and of early nineteenth-century England.

JOHN CLIVE
Harvard University

JULIA NAMIER. *Lewis Namier: A Biography*. New York: Oxford University Press. 1971. Pp. xvii, 347. \$13.75.

Superlatives in any critical review are always suspect. Yet it is difficult to avoid using such language in commenting on a biography that is in many ways unique. There was nothing conventional about Lewis Namier, and his widow, in writing about him, has heeded his injunction to tell the truth about everything. Whether speaking of Namier's relations with his parents, the women who entered his life, or the academic and political personages who figured in it at one time or other, Julia Namier has succeeded admirably in rendering the complexity and poignancy of many of these relations. The chapters on Namier's East European childhood and youth are remarkable, not least because they so obviously anticipate many of the difficulties that Namier was to experience as an adult. The summoning up of a vanished pre-World War I Polish agricultural society is almost incidental to a larger purpose—the consideration of how a boy, reared in these circumstances, could make England his own and, in time, come to be honored as one of England's most distinguished historians. The route

was never an easy one, nor did it lead straight to the fame that Namier knew mostly in his later years. Whether arguing about the settlement of frontier problems in East Central Europe in 1919 or defending Zionism a decade later, Namier was an isolated figure, always in touch with certain members of what would today be called the establishment, but never entirely a member of the club. The biography documents his passion but also the ambiguity of his position.

This work is remarkable for its candor, not simply about personal relations, which are rarely discussed so openly by a biographer, but also about the inner life of the man and the uncertainties that led him to despair and, on one occasion late in life, to the edge of suicide. Julia Namier knows how to blend the private woes of a spiritually troubled man with the public catastrophes of a time of tyranny without making either the occasion for maudlin observations about a civilization in decline. She knows how to render the health-giving properties of work without losing sight of the situations that even a conscientious commitment to work will not remedy. The man who emerges from this study is only partially recognizable in his histories; the woman who wrote this work has created a standard for biography scarcely less impressive than the one Namier so diligently sought to achieve in his histories. That is no mean accomplishment. Other generations will be grateful for this unique twentieth-century document.

STEPHEN R. GRAUBARD
Brown University

HAROLD MACMILLAN. *Riding the Storm, 1956-1959*. New York: Harper and Row. 1971. Pp. viii, 786. \$15.00.

HAROLD WILSON. *A Personal Record: The Labour Government, 1964-1970*. Boston: Little, Brown and Company. 1971. Pp. 836. \$15.00.

The dominant feature in these two prime ministerial memoirs is the primacy of foreign policy. Here are two men who ruled their country in periods of considerable social and political change, and they spend most of their time recording their diplomatic experiences. Their choice of emphasis is made all the odder by the fact that neither of them scored any long-term

diplomatic successes, and they suffered some very visible failures.

It has been suggested that Macmillan drifted into a greater and greater concern with foreign policy in the second half of his premiership, and this tendency may in the autobiography have spread back into the earlier half. The demands of cabinet secrecy may also have had something to do with it: presumably prime ministers spend a great deal of their time reconciling ministers who disagree with one another, but they may feel they cannot talk about these disputes unless they have already come into the open, usually through a ministerial resignation. This must be particularly important for Wilson, who is still working with most of his old cabinet colleagues, but probably Macmillan also feels the demands of loyalty.

As an honorable man, Macmillan has a number of problems about Suez: he cannot tell a lie about Anglo-French involvement with Israel, he cannot denounce his old colleagues, and he can hardly pretend it never happened. On the whole he chooses the last approach; he treats the whole crisis as though it were primarily a problem in Anglo-American relations. This solves a number of difficulties: he can show, relatively easily, that United States policy was not as comprehensible or as likely to avert the danger of war as it might have been; he can totally ignore, as though it was an incidental detail, the question of "collusion"; he can dwell on the restoration of good Anglo-American relations, and more or less ignore the weakening of Britain's position in the Middle East that followed the failure of the Suez operations.

His account of the failure of the first (1956-58) round of British negotiations about the Common Market is incomplete in a rather different way. In these negotiations the British government wanted a grouping of countries that would have industrial free trade but no common tariff and no unified agricultural policy. "Plan G," which outlined this position, would have suited Britain's needs very well, but there is no indication that Macmillan ever noticed how little it would have suited the needs of the French, who agreed to industrial free trade only because of the advantages they hoped to gain from a common agricultural policy. Macmillan condemns de Gaulle for rejecting Plan G; it is quite possible that de Gaulle's

tactics were devious, but they were not nearly as inexplicable as Macmillan makes them appear.

At this time his chief success was to lead the unwilling leaders of the United States, the USSR, and France to the Paris conference of 1960. As it turned out, nothing came of the conference, but it was certainly an interesting shift in international affairs. All the same, it is a little surprising that over one-eighth of the whole book is devoted to his visit to Moscow in 1959, especially as very little emerges that was not already known.

While all this was going on, people in Britain were moving from austerity to affluence. Macmillan was well aware of this at the time; he complains that his remark "You've never had it so good" was misapplied by his enemies, but he does not say what he thought about the changes of the late fifties. Perhaps his government simply decided to relax and let everybody enjoy the transformation. Macmillan disclaimed any desire to give moral leadership to the country and said that it was a problem for the archbishops, but it is not certain that the problem can be shrugged off quite so easily.

Fairly clearly Wilson would also like to avoid such difficult and intangible questions. It is quite possible that his premiership will be remembered mainly for the passage of liberalizing legislation on capital punishment, homosexuality, abortion, theatrical censorship, and so on, but practically nothing is said about these issues. Even comprehensive schools, which were part of the official program, get very little attention. Despite the title, this is as much a volume of memoirs as Macmillan's book: a more up-to-date, less histrionic account, but still an account of life seen from 10 Downing Street rather than the story of how the government did its work. There is of course a good deal more self-justification than in Macmillan's book, but then Macmillan made so few mistakes between 1956 and 1959 that he does not have to justify himself. Wilson is aware that he has a good deal to explain, and he makes a fairly good attempt at explanation. He certainly admits he made mistakes: small ones, on the less important issues, but his explanations are interesting and illuminating.

On the big issues, not surprisingly, he is less ready to admit mistakes. He explains why he

made the blunder of saying, after devaluation, "The pound in your pocket has not been devalued"; he does not really face all the problems involved in refusing to devalue for so long. It sounds as though Wilson now believes the pound was overvalued in 1964 (p. 6); the interesting question is whether he believed that in 1964.

Inevitably there is a great deal about the balance of payments. It is presented surprisingly unsystematically; most of the statistics are now public knowledge, and it might have been useful to print some of the tables and provide comments on progress over the whole six-year period. This would have run against the pattern of the book: Wilson tells his story in chapters about each two- or three-month period, which is very effective for showing how a prime minister's life is made up of a diversity of unrelated problems, but carries to an extreme the impression of absence of any continuity of policy for which Wilson's government was sometimes blamed.

Despite the emphasis on the balance of payments, the book clearly concentrates on foreign policy. Some of this may be for tactical reasons—the discussions with Mansholt about the Common Agricultural Policy (p. 343) help justify his opposition to British entry to the Common Market—but most of it seems to be the simple enjoyment of feeling that he was taking part in great events. His meetings with de Gaulle are very entertaining and very well told. His obvious dislike of Ian Smith may be put into higher relief to rally his Labour followers, but seems genuine and entirely justified. His meetings with Kosygin and with Johnson do not really do credit to anyone. It now seems fairly clear that neither Wilson nor Kosygin had much power to do anything about Vietnam, but they went on behaving to each other as if they could control the whole question. It is hard to avoid a faint suspicion that part of the reason was that they wanted to keep up their prestige in front of each other.

But it is also hard to avoid the suspicion that Wilson was being deceived by Johnson: a number of Wilson's best instincts, and also a number of his worst, combined to make him singularly vulnerable to Johnson, and there is every sign that Johnson took perfectly natural advantage of this. An overvalued British

pound was the first line of defense of the dollar; a Labour government that acquiesced in Vietnam was a useful item of diplomatic protection. Macmillan, or even Churchill, might have put up with such a role; the trouble is that friendship on such terms wears out, and the successor to Wilson as prime minister is a man cool enough about the United States to satisfy the discriminating taste of President Pompidou.

TREVOR LLOYD
University of Toronto

MAURICE MANNING. *The Blueshirts*. [Toronto:] University of Toronto Press. 1971. Pp. xi, 276. \$8.50.

MARTIN WALLACE. *Northern Ireland: 50 Years of Self-Government*. New York: Barnes and Noble. 1971. Pp. 192. \$7.25.

J. BOWYER BELL. *The Secret Army: The IRA 1916-1970*. New York: John Day Company. 1971. Pp. ix, 404. \$8.95.

Although Mr. Manning's study of the Blueshirt movement will undoubtedly enjoy its greatest circulation among specialists in twentieth-century Irish history, it deserves a more comprehensive readership. This detailed investigation of the rise and fall of Irish "fascism" in the 1930s should interest all students of the better known and more successful varieties of fascism of about the same period.

At its inception in 1932 the parent organization of the Blueshirts fit the familiar pattern of nascent fascism: ex-army officers, convinced of an alliance between the De Valera government and the "Communist" IRA and disturbed by the threat to free speech posed by government supporters' attacks on opposition speakers, formed a protection organization. Membership was broadened, and some of the more fashionable trappings of Continental fascism (including the colored shirt) were adopted. The author tells us that for two years, as the champion of the more affluent farmer during the economic crisis of the mid-1930s, the Blueshirt movement "dominated" Irish politics. Thus the movement's sudden collapse in 1934 and the disbanding of the remnant two years later make Mr. Manning's explanation of these events an important part of the book. The specialist in European history will certainly note the failure

of the Blueshirts to produce either effective leadership or a generally accepted ideology, for the ideas of the leaders, including the capital "F" fascism of a small group at the top, seem to have had little meaning to the rank and file. The author's claim that the movement failed to meet certain important criteria of interwar fascism and thus was not truly fascist is beyond dispute.

Mr. Manning has written a very good book about a "movement whose main importance lies in its uniqueness." This otherwise impressive study is marred only by a serious flaw in organization—the separation of the chronological narrative from the discussion of the movement's ideological development. The detailed study of ideology and its evolution is reserved for the penultimate chapter; some readers will prefer to begin the book there and then proceed to the opening chapters.

After reading Mr. Wallace's *Northern Ireland*, one is inclined to suggest that the Blueshirt leaders of 1934, seeking a model to follow, should have paid less attention to Italian *fascismo* and more to Ulster Unionism. During the 1920–70 period covered by the book, Unionists controlled the government at most levels and predominated in the police, the courts, and the civil service. The Unionist formula was very simple: encourage and play upon the fears of the Protestant majority while attacking the Catholic minority as allied with "hostile" outside forces.

Mr. Wallace has produced an informative, well-written, and unpretentious work, a handbook of Ulster politics that can be read in a single sitting. The core of the book is the evolution of Northern Ireland's political parties and institutions, and the changing relations among Stormont, Westminster, and Dublin. Unfortunately since the book's completion in mid-1970, the agony of Ulster has made the modest political and institutional reforms suggested by the author appear sadly inadequate.

Mr. Bell's *The Secret Army* is certain to be a more controversial book than Mr. Wallace's rather bland study of Northern Ireland. Some readers will almost certainly charge *The Secret Army* with being a one-sided glamorization of IRA violence; others may reply that the pro-army bias is never hidden, that the book is exceptionally well researched, and that glamoriz-

ing violence by historians has been going on for centuries. It is to be hoped that this debate and the excessive emotionalism of the author's conclusions will not obscure the one indisputable merit of the book—its detailed evaluation of the IRA as a revolutionary movement. It details one of the longest histories of revolutionary activity available, and "to dig into the history of the IRA is to uncover not only what to avoid but how to persist, to endure, to suffer disaster and to maintain the ideals and the organization" (p. 374).

GALEN BROEKER

University of Tennessee

J. H. WHYTE. *Church and State in Modern Ireland, 1923–1970*. New York: Barnes and Noble. 1971. Pp. xii, 466. \$13.50.

The extent to which the Roman Catholic Church can and does affect governmental policies in the Irish Republic is one of the most hotly debated issues of the current Irish imbroglio. Ulster Unionists charge that the southern government is a theocratic state which, if it ever gained jurisdiction over them, would promptly deprive them of their civil liberties at the bidding of cowed inquisitors. Catholic spokesmen reply that the Irish hierarchy is just another interest group like, say, the trade unions. John Whyte sets himself the task of assessing these divergent claims.

Nearly half of the book is devoted to a careful treatment of the background, course, and aftermath of the 1951 controversy over the so-called Mother and Child Scheme. Dr. Noel Browne, energetic young minister for health in a coalition government, collided head-on with the Catholic hierarchy over regulations to implement a health act of the previous government to which the bishops had expressed misgivings only after its enactment, and then only privately. Though the scheme's proposed state intervention into areas that might involve sexual morality was the most obvious source of friction, Whyte shows that the clash had much wider ramifications. Systematic Catholic social thought had come rather late to Ireland and, in the 1930s, had crystallized into a naively rigid and conservative form of the corporate thinking or "vocationism" expounded in Pius XI's 1931 encyclical *Quadragesimo Anno*.

Throughout the 1940s Whyte finds trouble brewing between Catholic spokesmen obsessed with finding ways to limit state power and civil servants reacting against repeated suggestions that their really rather modest power ought to be circumscribed by a host of consultative bodies representing "vocational" groups. Dr. Browne took the "bureaucratic" side on a particular issue and found himself opposed not only by a well-defined "vocational" group, the Irish Medical Association, but also by the concerted forces of the Church. His short but brilliant ministerial career was terminated following what looked very much like an ultimatum from the hierarchy.

Though the episode certainly lends credence to the "theocratic" interpretation of Irish politics, Whyte finds considerable evidence for the other interpretation. The Mother and Child crisis represented the apogee of episcopal intervention in the politics of the Irish state. In the 1920s and 1930s, to be sure, the Free State did enact a number of elements of the Catholic moral code into law, but there is little evidence that the bishops had to apply pressure to achieve this end. On the contrary, it resulted from "general agreement in Irish society on the necessity of such measures." Since 1960, moreover, there has been a marked tendency to relax the "integralist" pattern of law and administration which assumed that to be Irish was virtually to be Catholic, and the hierarchy has in general acquiesced in this tendency.

Whyte finds that the Church's position on a continuum between theocratic dictator and just-another-interest-group is affected not only by the circumstances of the time but by the degree of episcopal consensus and the nature of the issue at stake. The state and individual public figures can, indeed, treat the bishops rather cavalierly in cases where a specifically national interest is thought to be involved. In areas such as personal morality and education, however, the hierarchy's opinion carries much more weight. Indeed, deference to the Church's claims in education was so complete until the 1960s that this topic seldom appears in the book. As education usually bulks very large in discussions of Church-state relations elsewhere, this fact deserves perhaps a bit more attention than the author gives it. Whyte concludes that there is no simple answer to the question "How

much influence does the hierarchy possess in Irish politics?" but he provides a highly useful analysis of how that influence varies under the impact of different circumstances. Though denied access to ecclesiastical archives he has made valuable use of interviews and of a wide range of published materials. His judgment is invariably sound and his logic compelling. The book is required reading for anyone who hopes to understand contemporary Ireland and is the most important historical work yet to appear on the post-Reformation Catholic Church in Ireland.

DAVID W. MILLER
Carnegie-Mellon University

JEAN-PIERRE BARDET *et al.* *Le bâtiment: Enquête d'histoire économique, XIV^e-XIX^e siècles*. Volume 1, *Maisons rurales et urbaines dans la France traditionnelle*. (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches Historiques. Université de Caen, Centre de Recherches d'Histoire Quantitative. Industrie et artisanat, Number 6.) Paris: Mouton. 1971. Pp. 544. 78 fr.

This story of buildings and, to a lesser extent, the building trades is a series of five scholarly essays by members of the Centre de Recherches d'Histoire Quantitative de l'Université de Caen, whose director, Pierre Chaunu, has contributed an excellent introductory essay. It is a call to action for the current generation of historians. The message itself is not new, but it has become more insistent since the last war. We must attempt to see how the common people—those who were not politically, socially, or economically important in a prestatistical era—lived.

One of the most persistent qualities of these essays is their very tentative nature. Pierre Gouhier, for example, has investigated the cost of constructing the presbytery in eighteenth-century Normandy. He would also like to examine construction costs in general in the eighteenth-century countryside, but he has been hampered by the lack of meaningful data such as written contracts or agreements that clearly spell out the obligations of the various parties. Instead, Gouhier concludes, private contracts often were not recorded or, worse

still, were oral. Furthermore, the building industry in the *ancien régime* was often only one segment of a barter economy. The peasant might pay for all or part of the construction of his house by providing board and room or by giving the artisan part of his crop. On the other hand, many peasant families provided the manual labor themselves, calling in specialists—masons, carpenters, etc.—only for the finishing touches. The construction cost of a presbytery, which Gouhier equates with the house of a *laboureur aisé*, can be determined because the construction contract had to be approved by the intendant and ratified by the *Conseil du Roi*. Using this more dependable although restricted data, Gouhier discovered that construction prices in the Norman countryside doubled between 1750 and 1789, conforming to the general price increase. There is no precise way, however, of correlating prices in Normandy with rural construction in general.

Hugues Neveux has utilized the especially complete *fonds* at Cambrai to examine the cost of construction and maintenance from about 1400 to the beginning of the eighteenth century. The dramatic rise in the price of wood, particularly oak, as well as the diffusion of stone as a building material meant that fewer wooden houses were constructed after 1650. Stone buildings, constructed to last for centuries, were increasingly erected by all segments of society. "Houses of stone became less the exclusive property of the wealthy" (p. 256). Neveux has included a number of leases showing construction costs that date to the late fourteenth century; nevertheless, he estimates that he has examined only about one per cent of the houses of Cambrai. Chaunu compares Neveux's painstaking methods with the reconstitution of families undertaken by demographers.

In addition to Chaunu's introduction, there are also contributions by Gabriel Désert and Jean-Pierre Bardet. Of particular interest to the specialist are extensive original documents that help the reader to appreciate construction procedures, prices, wages, and so forth as far back as the fourteenth century.

Building was the most important nonagricultural economic activity in Europe until the second half of the nineteenth century, yet this subject has been generally neglected by historians and economists until recently. Chaunu and

his colleagues have done us a service by publishing the first results of their research and suggesting the direction such research may take in the near future. We can anticipate more studies in the future, which, one hopes, will allow us some comparison with the rather narrow investigations made in this book.

THOMAS F. SHEPPARD

College of William and Mary

J. SHIMIZU. *Conflict of Loyalties: Politics and Religion in the Career of Gaspard de Coligny, Admiral of France, 1519-1572*. (Travaux d'Humanisme et Renaissance, Number 114.) Geneva: Librairie Droz. 1970. Pp. 220.

The struggles between Guise and anti-Guise, Catholic and Huguenot noble factions to gain control of the French Crown in the Wars of Religion have fascinated scholars and led to a range of historical interpretation. Whereas historians in the nineteenth century emphasized the religious basis of the struggle, recently the tendency has been to stress the political issues and personal ambitions hidden under the cloak of religion. Dr. Shimizu and others believe that neither interpretation does justice to the complicated interplay of religion and politics. In this first study of Coligny in almost seventy-five years the author emphasizes the subtle intermingling of religious and political motives and demonstrates the conflicting interests and loyalties that buffeted the French nobility.

Beginning with Coligny's youth and first known contacts with the Reform, Dr. Shimizu focuses on Coligny's role at court under the Guise regime and his gradual emergence as a leader and spokesman for the Huguenot party. She concludes that Coligny's fundamental motives were political; indeed she finds evidence of personal ambition to be the consistent element in Coligny's assuming a leading role in the Huguenot movement. She sees him as a statesman, primarily, and not a man of religion. She convincingly argues that while he loyally worked for the Protestant cause, he remained indifferent to questions of religious dogma because his principle concern with religion was its role in maintaining social order and justice. Coligny's affiliation with the Bourbon-Protestant faction and his public declaration for Protestantism

were thus well-timed acts of political self-preservation.

This study, modest in scope and careful in its scholarship, is not totally convincing in its conclusions. On the question of conflict of loyalties it is difficult to accept Dr. Shimizu's formulation of a conflict between religion and politics. The assertion of a conflict suggests the existence of a tension between Coligny's loyalty to the Crown and to Protestantism. Yet she makes no doctrinal case for Coligny's loyalty to the Reformed religion. The evidence she presents suggests only that Coligny single-mindedly pursued a career aimed at becoming first servant of the Crown and joined the Huguenot movement to advance his political fortune with anti-Guise factions organized to free the Crown from Guise control. Coligny was undoubtedly a religious moderate, as Dr. Shimizu argues, and it is valuable to have his political ambitions brought into focus at last. But there are points at which Coligny remains shadowy and one-dimensional. The picture of conflicting loyalty might have been sharpened if she had also considered Charlotte de Laval's spiritual influence on her husband, Coligny's correspondence with Calvin and Beza, his friendship with Renée de France, and the role of the Protestant pastor in Coligny's household.

Dr. Shimizu's scholarship is impressive: her archival evidence is precise. She has certainly examined every known letter and diplomatic dispatch and uncovered new evidence to support her argument. However, the information she has amassed appears at times to have overwhelmed her. There are several instances where lengthy digressions on questions of interpretation might have been relegated to footnotes. Elsewhere quotations could have been digested and paraphrased. But the questions she raises about political and religious motivation are important and should be applied to other personalities at Catherine de' Medici's court. Certainly Dr. Shimizu's answers help to clarify our understanding of the confused religious and political scene in sixteenth-century France.

CHARMARIE JENKINS BLAISDELL
Northeastern University

EDMUND H. DICKERMAN. *Bellièvre and Villeroy: Power in France under Henry III and Henry*

IV. Providence: Brown University Press, 1971. Pp. x, 200. \$10.00.

Considering the extensive research upon which it is based, this book makes only a very modest contribution to the monographic literature on the reigns of Henry III and IV. Unfortunately the choice of an artificial, routine subject did less than justice to Professor Dickerman's painstaking perusal of the sources, many of which are unpublished.

The ostensible purpose is to "recreate the Weltanschauung" of these two important royal ministers by a topical presentation of their opinions on various subjects as expressed in their voluminous correspondence. Grouped into three bulky chapters (on royal government and foreign and domestic policies) are assorted nuggets of information on, for example, their reactions to dismissal by Henry III in 1588, their conventional sentiments on the horrors of war, compassion for the peasantry, acceptance of the social structure, or their concern about Henry IV's recklessness in battle. The justification for such an anthology is the assumption that Bellièvre and Villeroy "spoke for an important segment of what we today call the enlightened public." Little attempt is made, however, to keep the opinions firmly rooted in their respective historical contexts, or to compare them with differing or conflicting ideas. Divorced from the detailed, concrete circumstances to which they refer, their utterances tend to sound merely inane.

The tone, moreover, is so adulatory as to border on the hagiographic. Dickerman minimizes the overwhelming evidence that Bellièvre began to sink into illness and senility soon after his appointment as chancellor in 1599. And one would never guess that Villeroy, "the dean in chapter of all the statesmen in Christendom" (as Carew called him in 1609), was noted for his ostentatious scale of living and was rumored to be angling for a cardinal's hat.

Even this *table méthodique* of ministerial commonplaces would have been more useful and accessible to the scholar—the book cannot seriously be intended for any other audience—were it not for the unfortunate practice of tucking the references out of sight in unwieldy omnibus footnotes at the end of the volume, thus presenting the reader with a

series of nasty little "matching tests" (some of which I confess I found insoluble).

Why these two particular royal ministers? Why not Sully, Sillery, Jeannin, Du Perron, and de Thou? They are all virtually eliminated here. I am afraid that Dickerman's book is no substitute for a biographical approach. It adds little to Kierstead's study of Bellièvre or to Sutherland's work on the secretaries of state; and for the last twenty years of Villeroy's remarkable half-century career, we are still left with the old and grossly inadequate biography by Nouaillac.

ALFRED SOMAN
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MOHAMED EL KORDI. *Bayeux au XVII^e et XVIII^e siècles: Contribution à l'histoire urbaine de la France*. (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences Économiques et Sociales. Centre de Recherches Historiques. Civilisations et sociétés, Number 17.) Paris: Mouton. 1970. Pp. vii, 369. 48 fr.

Let us welcome eighteenth-century Bayeux into the magic circle of French cities that have inspired more than one solid study. In 1967 Olwen Hufton published a careful description of Bayeux during the last quarter of the eighteenth century; that study contained important information about the city's administration under the Old Regime and the early Revolution but slighted the analysis of population composition, demographic change, and city-hinterland relations. Mohamed El Kordi's *Bayeux* devotes only four or five pages to administrative matters and stops at the beginning of the Revolution; but this study runs back to 1600, deals at once with the city and its region, and provides exquisite detail on demographic structure, prices, wages, agricultural production, plus a dozen more of the items now conventionally quantified in French regional studies. El Kordi does all the analyses we have learned to expect of books issuing from the École Pratique des Hautes Études: family reconstitutions, long series of prices, studies of occupational composition, and so on. As usual, we have to endure stylish substantives like *le dynamique* and *l'historique*. And the design of the whole book is also familiar: (1) sources, (2) composition of the population, (3) demo-

graphic changes, (4) economic structure and, especially, change, (5) appendixes, mainly statistical. Every part is competent and jam-packed with information. El Kordi has followed a classic design and has done his duty by it.

The classic design has its costs. Among other things it tends to diffuse the book's argument. The student of a particular city or region finds himself in something like the position of the nineteenth-century author of a local *monographie*: compelled to fill a series of prescribed blanks whether or not his locale, his evidence, his subject, or his own proclivities lead him in those directions. For most of its way El Kordi's book lurches from description to description, unaided by a general argument. The conclusions El Kordi himself draws from his work are mainly descriptive: that irregularly declining mortality and high levels of immigration (especially of women) accounted for the city's growth in the seventeenth and eighteenth centuries; that people of the region were probably exercising deliberate control over births fairly early in the eighteenth century; that the city and its region alike normally had to import food to survive, a general shift toward stock-raising increased the need for imports, and the urban poor suffered increasingly from the high food prices that resulted from these circumstances; that Bayeux's lace industry, despite its employment of a considerable number of local women, never had great prospects and never played a major part in the region's economy; that at Bayeux, as elsewhere, the "long" seventeenth century (up to 1720 or so) saw relatively little change in general price, wage, and income levels, while the last three quarters of the eighteenth century (especially the period after 1760) brought important rises in prices and rents, thereby depressing the real incomes of the wage-earning segments of the population. For the most part he documents these conclusions amply, carefully, and appropriately. An encyclopedic account of a middle-sized city, of its hinterland, and of its *rentiers*, monks, merchants, lacemakers, and poor folk takes its place on the shelf, awaiting comparison with other cities of the seventeenth and eighteenth centuries.

CHARLES TILLY
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A. LLOYD MOOTE. *The Revolt of the Judges: The Parlement of Paris and the Fronde, 1643-1652*. Princeton: Princeton University Press. 1971. Pp. xiv, 407. \$15.00.

Lloyd Moote's richly detailed, meticulously documented study of the mid-seventeenth-century "judges' revolt" provides us with one of our first authoritative accounts in English of the Old or Parliamentary Fronde and the succeeding civil wars in France. The title and the initial date of the book are misleading: Moote discusses not only the Parlement of Paris but the rise and decline over a fifty-year period of a broadly based "royal opposition" drawn from the ranks of the established bureaucrats (*officiers*) and of the well-to-do middle classes. Eloquent in their discontent, the older establishment feared the usurpations of newly appointed bureaucrats (*commissaires*), dreaded the loss of income through spiraling taxes, and, after 1643, deplored the bumbling mismanagement of the regency government headed by Anne of Austria and Cardinal Mazarin. Moote is severe in his judgments of the regent and her advisers: Anne "did not have the perseverance, patience, or intelligence" to guide the government (p. 68); Mazarin had "totally lost control due to his ignorance and inattention" (p. 87); Particelli d'Emery's financial schemes were tainted by corruption; and Chancellor Séguier lacked "Richelieu's firmness and tact" (p. 69). After five years of the regency's misrule a storm or revolt broke over Paris in the summer of 1648 at a time when the sovereign courts were sitting in joint conclave to draft petitions for redress of grievances. These grievances, codified in the famous Twenty-Seven Demands, urged, *inter alia*, the suppression of the office of provincial intendant, limitations on the arbitrary legal powers of the Crown, and an adjustment of the tax rates. In the confusion of ensuing civil wars the constitutional-administrative issues were all but lost sight of. Moote insists, however, that the judges did achieve "victory-in-defeat" by slowing down the advance of royal absolutism and by forcing reforms on the government of Louis XIV. "Without the political *via media* [urged by the judges], the Fronde would have been a total failure" (p. 376).

Moote's accomplishments are impressive: he has given order and emphasis to recent interpretations of the Fronde; he has probed, with

insight and learning, the aims and achievements of the officers; and, above all, he has provided the judges with an updated *apologia pro vitis suis*.

Moote elucidates the royalist cause with less deftness than he does that of the *officiers*. It is difficult, for example, to penetrate the various guises taken after 1643 by the so-called *conseil d'état* and by the emerging "inner council," the latter probably being the *Conseil d'en Haut*. Moote might also have discussed roles played by the refurbished *conseil privé* and (after 1648) of the *conseil des dépêches* in their struggle with men of parlement. The portrait of the principal minister is strikingly unflattering: to say that Mazarin lacked "Richelieu's ability [in foreign affairs] to distinguish between desirable goals and attainable objectives" (p. 67) ignores the tangible gains won by Mazarin at the treaties of Westphalia; to say that "his chief weakness . . . was his incredible ignorance about internal affairs" overlooks the minister's considerable abilities as a bureaucrat; and to say that he and Anne "encouraged the upheaval [of the Fronde], just as Charles I of England and . . . Count-Duke Olivares [of Spain] had incited revolution in their countries" (p. 87) strains an analogy. Ultimately Moote might have given us a clearer and more extended discussion of royal absolutism; such a discussion seems essential to our understanding of an age in which maxims of state were hotly debated, in which the fate of a law turned on phrases like *qui veut le Roy si veut la loy* or *Le prince . . . est une puissance publique* and in which the interpretations of *legibus soluta* and *lex regia* formed the background to civil war.

JOHN C. RULE

Ohio State University

MICHEL MORINEAU. *Les faux-semblants d'un dé-marrage économique: Agriculture et démographie en France au XVIII^e siècle*. (Cahiers des Annales, Number 30.) Paris: Librairie Armand Colin. 1971. Pp. 387. 45 fr.

This book includes two major articles (140 pages), and a long appendix (240 pages). The latter is a collection of diversely edited, annotated, or interpreted documents: tithe returns, agricultural and commercial inquiries, and a farmer's account. In the first major article, an

expanded version of a piece that appeared earlier in the *Revue Historique* (1968), and was included (in translation) in *Essays in French Economic History* (Rondo Cameron, ed., 1970). Morineau questions the existence of an "agricultural revolution" in France in the eighteenth and early nineteenth centuries. He compares the agricultural census of 1840 with quantitative evidence on yields and productivity for the early eighteenth century. To this is added an abundant but scattered collection of evidence pointing to the same conclusion: that yields of cereals per acre or per bushel of seed stagnated during the period in question. This is excellent work, and the findings are remarkable. But to conclude from this that there was consequently no agricultural revolution underestimates the importance of new crops, particularly maize and the potato. Morineau dismisses this objection by arguing that such new crops did not truly constitute an agricultural revolution since they were not accompanied by rising incomes: the potato was a symptom of poverty rather than prosperity. But is this not begging the question?

Having argued for the absence of an agricultural revolution in France in the period 1700–1840 Morineau then argues that this casts a doubt on the existence of a "demographic revolution," since the former is supposed to have caused the latter, and he sets himself the task of justifying his doubts (the second major article). A long discussion leads him rightly into stressing diversity in the demographic history of French regions. Differences in rates of population growth in the eighteenth century are linked to the different relations between population and means of subsistence existing in the various regions at the start. He finds that, where he estimates there were large food surpluses, population growth was more rapid. On the other hand, he recognizes that rapid population growth could also be sustained on other rural, but nonagricultural sources of income, such as cottage industry. However, Morineau finds two exceptions, Languedoc and Normandy, to the positive long-run association between population growth and rural industrialization.

At this point (p. 333) he throws up his hands, and the discussion ends abruptly with the unexpected conclusion that the sources of

regional demographic differences must be sought on the side of changing patterns of epidemics and contagions (i.e. unexplained exogenous forces). The study ends with a conclusion that there was neither a demographic nor an agricultural "revolution" in France until 1850 or so, only regional variations in the degree of stagnation.

An unfortunate defect of this book is its writing style. The reader is drawn into an interminable monologue with exclamations and interrogations as well as allusions, archaisms, neologisms, and colloquialisms. Going through all of that is thoroughly exhausting. Nonetheless, the primary and secondary material that the book contains will make it indispensable for specialists of French agricultural or demographic history in the period 1700–1850.

FRANKLIN MENDELS
University of Quebec

PATRICE L.-R. HIGONNET. *Pont-de-Montvert: Social Structure and Politics in a French Village, 1700–1914*. (Harvard Historical Studies, Volume 85.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xv, 217. \$8.50.

With a sure but unobtrusive display of factual and statistical erudition based on impressive combing of national, local, and family archives, Professor Higonnet has pieced together some political and social aspects in the modern history of a village in Southern France. In the process he develops interesting generalizations about the some thousand souls inhabiting the cluster of twenty-two hamlets and central bourg, while setting the isolated community in the historical and historiographical context of France as a whole. Stressing a tripartite social structure of rural and urban "lower class," landowning peasants, and rich peasant and bourgeois "notables," the author sees a Porchnev type of late seventeenth-century class conflict insofar as the elite Protestant Montipontins stood aloof from their social inferiors' Camisard wars with Catholic state authority. Using the same social base he then shapes the community-wide support of the French Revolution around a Cobban-like theme of a bourgeois elite strengthening its traditional power. Finally he traces the results of the nineteenth-century "breakup" of the social structure: the petit bourgeois, who stayed on while the top

and bottom elements vanished, proceeded to engage in radical political ideas removed from the concrete issues of their little world while socially clinging somewhat conservatively to their impoverished way of life.

Though Higonnet does balance these bold outlines with careful sifting of details, some basic questions remain. First, the village was untypical insofar as it had no resident nobility, and its pervading Protestantism also made it less typical than the author suggests. The second objection is to the book's general, though not consistent, emphasis on social structure as a key to the events of Pont-de-Montvert's history. For example, it is straining the implications of the social elite's inaction during the Camisard wars to see this as a class conflict between rich and poor. In the case of the French Revolution, the scraps of political records left for the historian do not allow much to be said of political and social interaction. And the postrevolutionary chapters combine the assumption that "attenuation of social distinctions" caused doctrinal unanimity with the quite different stress on the impact of education on society and politics.

A. LLOYD MOOTE
University of Southern
California

IRA O. WADE. *The Intellectual Origins of the French Enlightenment*. Princeton: Princeton University Press. 1971. Pp. xxi, 678. \$20.00.

This lengthy book is the first of a projected three volumes devoted to "the reality of the Enlightenment: its origin, its development into an organic something, its consequences" (p. x). Professor Wade, well known among students of French literature for his important works on Voltaire, fears that while we know more about many particular aspects of the Enlightenment than we did fifty years ago, we may also know less about "its inner nature, its personality, its internal reality" (p. xiii). His ambition is to synthesize the partial investigations of recent decades in such a way as to reveal the true Enlightenment: the organic spirit of the age. That such an organic spirit exists (or existed), Professor Wade has no doubts. He is therefore perturbed by the fact that historians have apparently foisted their "private Enlightenments"

upon the public as authentic. It may be, however, that the conscious discipline of making a "private enlightenment" public knowledge is essential to good historical writing. For Professor Wade's attempt to analyze the origins of the one true Enlightenment has produced a book that is exasperating in its pretensions, eclectic in its method, and confusing in its logic.

Professor Wade starts his book with an introductory section entitled "Enlightenments We Have Known." The first chapter, devoted to "The Changing Picture of the Enlightenment," deals instead with political and social conditions in eighteenth-century France. The burden of the argument is that historians now regard the period as more prosperous than they once did. But the exact relevance of this interpretation to the French Enlightenment is not made clear, while Professor Wade's conclusion that we have overlooked "the fact that, for Enlightened Man, it is thought which leads the world" (p. xiv) seems largely gratuitous. The second chapter of this introductory section gives a preliminary characterization of the Enlightenment for which Professor Wade is seeking origins in this volume: the Enlightenment as defined by Kant and Cassirer, for whom the problem of knowledge was the critical issue. Following the work of Henri Busson, Professor Wade traces this problem back to the Italian naturalism of the Paduan school and above all to Montaigne, whose "Que sais-je?" was to become the constant watchword of the free-thinkers and the constant challenge to the thinkers (i.e. the philosophers).

Professor Wade then marshals a rich parade of free-thinkers and thinkers in three columns: Renaissance, baroque, and classical. There is much erudition here and much fascination. But there is little that is conclusive. There is, for example, a most interesting series of vignettes of five free-thinkers (Charron, La Mothe Le Vayer, Naudé, Patin, and Sorbière). But when Professor Wade asks exactly what these writers contributed to the making of Enlightenment thought (pp. 201-06), the question gets lost in a discussion of the views of the literary historians upon which he has drawn. More crucial is the lengthy section (the longest in the book) on "The Structuring of Enlightenment Attitudes" (pp. 418-643). In effect this is a discussion of five philosophers (Malebranche,

Leibnitz, Locke, Newton, and Bayle) with a brief excursus on the battle of the Ancients and Moderns and an anomalous coda on movements for reform under Louis XIV. Again, in themselves each of these discussions repays reading, for Professor Wade has read widely and thoughtfully. But taken together they do not amount to an analysis of the "structuring of Enlightenment attitudes" in any sense. Perhaps this is because Professor Wade distinguishes between structure and form, the spirit of the age developing only as "structure" takes on "organic form." But it means that many readers may remain puzzled by this book until Professor Wade's intentions become clearer in its sequel.

K. M. BAKER
University of Chicago

VINCENT W. BEACH. *Charles X of France: His Life and Times*. Boulder, Colo.: Pruett Publishing Company. 1971. Pp. xv, 488. \$16.50.

In this study Beach has brought together research that extends over more than a decade. Some of it has been published previously, but there is much fresh material to help us better understand the contribution of Charles X (1757-1836) to the royalist counterrevolution in France.

Thirteen chapters span his life. After exploring neglected years in the dissipated court of Louis XV the narrative follows the tedium and fantasies of twenty-five years of exile and helps us to understand the problems inherent in Charles's assumption of the Restoration kingship at the age of sixty-seven. New information clarifies developments in three major areas: Charles's role in the Assembly of Notables as opponent of reform, the rationale for the coronation ritual and legislative program of his first three years as monarch, and the program and collapse of the Polignac ministry.

Throughout each of these crises and over a forty-two year period, the author remarks a stubborn consistency in Charles's political behavior. The political ideas spelled out in Charles's defense of aristocratic and monarchical privilege in the Assembly of Notables during the "pre-Revolution" remained his program for the active years of counterrevolutionary insurgency (1789-1804) as well as for

the decade of furtive conspiracy that followed. And, despite expedient and sporadic support for the Charter during the Restoration, these ideas emerged again in the July decrees that precipitated the revolution of 1830. For Beach this consistency is more stubborn blindness than statesmanship, but he does recognize the king's personal involvement in an ambitious foreign policy, a successful resolution of the contested revolutionary land settlement, and the marked economic progress of the Restoration.

One of the reasons why we have not had a scholarly biography of Charles before this has been the difficulty of gathering relevant sources. It is to Beach's credit that he has been able to unearth important unpublished primary materials in special collections at the Public Record Office and the British Museum as well as in important public collections in France. These were supplemented by newspapers, records of parliamentary debate, and a large memoir literature (much of which is unfortunately either hagiographic or, as in the case of de Boigne, maliciously anecdotal). The secondary works drawn on for authority are also substantial, although they are often introduced rather ponderously through lengthy citations offered to the reader without comment. With the exception of the years 1805-13, every period of Charles's life is illuminated in some way by these materials.

A second problem for scholars has been the dullness of the man, projected to liberal critics in his own time and to posterity. Charles was a far cry from his illustrious ancestor the Sun King. Beach has overcome an understandable aversion produced by Charles's early philandering, intellectual mediocrity, and procrastination to present a fuller picture than we have previously had of the last Bourbon monarch of France.

DANIEL P. RESNICK
Carnegie-Mellon University

ROGER PRICE. *The French Second Republic: A Social History*. Ithaca: Cornell University Press. 1972. Pp. vii, 386. \$11.50.

Not since Georges Renard contributed his volume to Jean Jaurès's *Histoire socialiste* almost seventy years ago has there been a serious social

history of the Second Republic. And this despite the best efforts of a historical society specifically devoted to the revolution of 1848, despite a centenary celebrated amidst orgies of retrospective studies and despite a modest stream of substantial publications since 1948. Professor Price's book seeks to fill this gap.

As a social historian Price is chiefly concerned with the linkage of French society to French politics, of social structure to political dynamics. The study divides naturally into three sections of comparable length: an introductory static analysis of French society prior to the February Revolution (pp. 1-94) that distinguishes between basic structure and the disruption peculiar to the period just preceding the revolution; second, a socio-political account of the revolutionary period *par excellence* (February through June 1848) of which over one-third is devoted to the social analysis of the June insurrection (pp. 95-192); third, a much sketchier account of the two and one-half years separating June 1848 from December 1851 (pp. 193-326) with, again, one-third devoted to the social analysis of the coup d'état. This adds up to a highly ambitious agenda. Price has unfortunately insisted on interspersing a running commentary on Marx's views of the Second Republic's social evolution that might have made an excellent article but tends to clutter up the book.

How successful is Price's social history? Very successful in some respects and certainly very useful, particularly to nonspecialists. For one thing the author's focus is on France and not, as has been customary, on Paris. For anyone interested in a scholarly assessment of French society prior to 1848 based on the best recent regional studies, Price's introductory section is authoritative. Furthermore he is genuinely sensitive to the variety of French society and hence to the complexity of its political response. The social significance of key episodes, such as the June Days and the resistance to the coup d'état, are more fully explored than in any other comparable work. Finally, considering its broad scope, the book is remarkably free of errors.

Yet I have misgivings that I find hard to articulate, some of which have to do with style and, by implication, organization. Historians do not spoil each other in this regard, but

Price's style runs from limp to soggy, with an occasional impenetrable sentence thrown in as additional challenge. The result was that I was so busy cutting my way through the underbrush that I was rarely aware how the forest was laid out. I am sure that each chapter does have an underlying outline, but it is not made apparent to the reader. Perhaps it is also the style that gets in the way of Price's overall interpretation. After giving the book two careful readings I remain unconvinced that any definable relationship between social structure and political behavior emerges. The author, following J.-A. Tudesq, begins by presenting France as a traditionalist society led by its notables, leading Price to an essentially determinist verdict on the revolution of 1848: conservatism was bound to prevail where so many had a stake, however modest, in the existing order. Yet in accounting for the radical sweep of the area of small peasant property in 1849 and the triumph of conservatism where large-scale agriculture deprived the rural masses of proprietorship, Price jettisons his earlier explanation. I guess I am worried by what I see as essentially circular reasoning. Because, for example, the Limousin votes socialist in 1849 the author is bound and determined to find reasons why the social conditions of the area make this inevitable. He would do so with equal gusto had it voted conservative. Perhaps social history must develop more sophisticated hypotheses.

One problem may be that Price does not marshal the full range of available evidence against which hypotheses ought to be tested. Price does best in his introductory chapters where he pulls together and displays the findings of some of the major recent French scholars in the field. Where he relies on his own, necessarily cursory archival research—namely for the period of the Second Republic itself—he depends additionally on standard memoirs and general histories, completely ignoring a mass of local and regional histories of the Second Republic (admittedly of unequal value). As it is, for his account of regional variations Price relies on the thin diet of the reports of the *procureurs généraux* and a patchwork of massive and recent doctoral theses that leave much of France blank.

Finally, I was disturbed by Price's carelessness in computing social data. Though I am

much impressed by quantification as a tool of social history, the quantifiable is not always what it seems. To generalize, as Price does, about the social composition of the June insurgents from the roster of 11,642 suspects arrested requires assurance that the bulk of them were indeed participants. Even an occupational analysis of the four thousand found guilty must take into account the district-by-district incidence of the repression, the industrial geography of Paris, and similar variables. A breakdown by birth place may prove equally treacherous: Do the 449 arrests of suspects born in Moselle speak for a special Mosellois propensity to rebel? Or were Mosellois in rebellion-prone trades? Or did they just happen to lodge in some neighborhood, like the Faubourg St. Antoine, subject to unusually severe repression? Quantifiable data have to be scrutinized by conventional historical methods just as much as any other findings. It does not, alas, speak for itself.

In summary, Price is successful in providing a cogent summary of what we know about French society at mid-century and in detailing the strains within it. He is less successful in relating social change to the nature and direction of political dynamics, but this is, of course, an extraordinarily difficult task. The publisher, incidentally, should be awarded the Legion of Demerit for inventing the most inconvenient method of footnoting yet devised by the ingenuity of man.

PETER H. AMANN
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HELMUT RUMPLER. *Ministerrat und Ministerratsprotokolle 1848–1867: Behördengeschichtliche und aktenkundliche Analyse*. With a foreword by FRIEDRICH ENGEL-JANOSI. (Die Ministerratsprotokolle Österreichs und der österreichisch-ungarischen Monarchie 1848–1918. First Series: Die Protokolle des österreichischen Ministerates 1848–1867. Einleitungsband.) Vienna: Österreichischer Bundesverlag für Unterricht, Wissenschaft und Kunst. 1970. Pp. 131, 4 plates. Sch. 498.

HORST BRETTNER-MESSLER, editor. *Die Protokolle des österreichischen Ministerrates 1848–1867. Part 6. Das Ministerium Belcredi. Volume 1, 29. Juli 1865–26. März 1866*. With an introduction by FRIEDRICH ENGEL-JANOSI. (Die Ministeratsprotokolle Österreichs und der österreichisch-

isch-ungarischen Monarchie 1848–1918. First Series, part 6.) Vienna: Österreichischer Bundesverlag für Unterricht, Wissenschaft und Kunst. 1971. Pp. lxxxiv, 351. Sch. 950.

The protocols of the Austrian ministerial councils have long been recognized as an important source for Austrian history and have been profitably used by many scholars. A joint Austrian and Hungarian project is in progress to publish these documents in their entirety. The first series (1848–67) is being compiled by Austrian scholars, the second (1867–1918, excepting the period 1871–83) by Hungarians.

Two volumes have now appeared in print. Helmut Rumpler's slim but valuable introduction to the first series, *Ministerrat und Ministerratsprotokolle*, presents a cogent summary of the role and significance of the Council of Ministers. Rumpler, the general editor of the project, also discusses the organization and the guiding principles adopted for the publication of the documents—as well as some of the peculiarities and limitations of the *Protokolle*. Waltraud Heindl has added useful tables of the personnel of the various ministries, 1848–67. The first volume of documents, compiled by Horst Brettner-Messler, covers the first eight months of the ministry of Richard Belcredi (July 1865–March 1866). The redaction is superb and will enhance both the ease and value with which the documents can be used. The volume is nicely indexed and includes a table of contents for each protocol and a list of all participants in the councils. A bibliography of pertinent secondary works for the period is added (though it is hardly comprehensive). Each protocol is printed in full, and there are explanatory and reference notes by the compiler. Friedrich Engel-Janosi has contributed a detailed introduction.

The fifty-six protocols printed here indicate clearly the limited authority exercised by the *Ministerrat*. The overriding concerns for Austria in these months were the Hungarian question and the contest with Prussia in Germany. Yet the council touched only sporadically and unsystematically upon the Hungarian issue and upon foreign affairs hardly at all; only three times was foreign policy even discussed. On the other hand, a wide range of economic and social concerns were brought before the ministers. The financial plight of the Empire was a

persistent and formidable problem and, as these documents reveal, was an added imperative for the resolution of the Hungarian and Austrian constitutional questions. The loan of 1865, finally negotiated with French bankers, came under such disadvantageous terms that it was openly described in the session of November 7 as "a swindle."

Conversely, this was also a period of economic expansion, and considerable information on the gradual industrialization of the Empire is available in the protocols. The extension of the railway network, the formation of a joint stock company, and numerous discussions concerning taxation policy and the promotion of various industries can be mentioned in this regard. There is also some information of use and interest on social and cultural developments in the monarchy—religious affairs, press relations, and educational questions all received attention.

As historical sources, the protocols of the *Ministerrat* have obvious limitations; the council shared the shadow rather than the substance of power. Nevertheless, anyone interested in the Habsburg Empire should welcome the appearance of further volumes in this project, which has been impressively inaugurated here.

RICHARD B. ELROD
Illinois State University

SAMUEL BERNSTEIN. *Auguste Blanqui and the Art of Insurrection*. London: Lawrence and Wishart. 1971. Pp. 364. £4.00.

A good biography of Blanqui is needed. Unfortunately Dr. Bernstein's effort, while offering some useful new information and interpretation, does not fulfill the need. The author rightly criticizes much existing work on Blanqui, both the calumnious tradition of conservative observers and a tendency among French communist writers to treat Blanquism as essentially a precursor of the Marxist movement. Utilizing an extensive manuscript collection on Blanqui, Bernstein offers a variety of personal details, including material on the huge portion of Blanqui's life that was spent in prison. Bernstein presents new material on Blanqui's judgment of events in which he participated, such as the revolution of 1848,

and the author has new insight into Blanqui's political thought, stressing the conspirator's belief in the motive power of ideas and a hostility to religion and superstition that outweighed his eloquent denunciation of capitalism. Though the overall interpretation of Blanqui is familiar, he is placed more understandably in the French intellectual and political tradition. Blanqui's failure to rouse mass support is also intelligently assessed.

The approach is largely descriptive and chronological. One aside on Blanqui's psychology misfires badly. Even the description is often confused by inserted accounts of general developments in French history—developments quite familiar in outline and based on limited and often dated secondary accounts. This pattern is less pronounced in the section of the book dealing with Blanqui's activities after the 1850s. Here the Blanqui manuscripts are more detailed and revealing, and the author is more content to assume that his readers know something of the general background. Even here, however, we are treated to a number of simplistic judgments of the Second Empire and the French working class.

The descriptive approach involves considerable repetition of interpretive points. We are told and retold of Blanqui's romanticism and his inability to communicate with workers. A few other judgments are repeated without significant proof; thus the claim that Blanqui was a racist and an anti-Semite. Blanqui seems constantly to be held up to a model and found wanting: he lacked a general theory of history, he did not properly understand class structure, he was a French nationalist in the Jacobin petty bourgeois tradition, he was conspiratorial rather than systematic. He was not Marx.

The book presents some information on the formation of the Blanquist movement in the 1860s. Its career under the Third Republic is sketched, but there is no overall assessment of the impact of Blanquism on later protest movements. In sum, the book should be read by anyone interested in political and social agitation in nineteenth-century France. But one must hope that it will not impede a more analytical and broadly researched study of this archetypal revolutionary.

PETER N. STEARNS
Rutgers University

DAVID STAFFORD. *From Anarchism to Reformism: A Study of the Political Activities of Paul Brousse within the First International and the French Socialist Movement, 1870-90*. [Toronto:] University of Toronto Press. 1971. Pp. xiv, 367. \$15.00.

Studies of nineteenth-century French anarchist and socialist movements generally treat Paul Brousse (1844-1912) as a figure of secondary importance and his *Politique des possibilités*, his Possibilism, as a species of political opportunism. In this carefully researched and clearly written account Dr. Stafford has endeavored to give Brousse his due, documenting his "crucial role" within the anarchist Jura Federation, the congresses of the First International, and the post-Commune French socialist movement, as well as seeking to establish a "logical line of development" that links Brousse's early anarchism with his Possibilism and to identify him as a realist and pragmatist to whom "action was the guide to theory."

During the period of his exile in Switzerland (1873-78), Brousse was indeed an intransigent, left-wing anarchist and a prominent exponent of "propaganda by the deed." The increasing isolation and general ineffectiveness of the anarchists, combined with his growing concern with the revival of the socialist movement in France, led Brousse to re-examine his doctrinal commitments. When he returned to France in 1880 he had considerably modified his revolutionary posture and was prepared to make use of legal means of action, the ballot and electoral campaigns, within the existing structure of bourgeois political institutions in order to transform society. Believing that an anarcho-communist society could be achieved in piecemeal fashion through the enactment of significant socialist measures on the local level, in municipalities and communes, Brousse and his followers gained control of the recently formed French Socialist party and, subsequently, replaced its Marxist-inspired Minimum Program with one that emphasized specific, practical, and moderate reforms. During the 1880s the Possibilist party, in contrast to the Guesdist and Blanquist parties, scored at the polls, winning seats on the Paris Municipal Council and in the Chamber of Deputies. Brousse himself became president of that council in 1905 and a deputy the following year, although his party

entered a period of rapid decline after the schism that occurred at the Chatellerault Congress in 1890. This fine study is based on a number of collections of manuscript materials located in Amsterdam, Paris, Bern, Brussels, Imola, and elsewhere, private papers in the possession of the Brousse family, relevant anarchist and socialist newspapers and periodicals, as well as a wealth of other primary and secondary sources.

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JAMES HARDING. *The Astonishing Adventure of General Boulanger*. New York: Charles Scribner's Sons. 1971. Pp. xii, 251. \$8.95.

It would be an act of charity to omit reference to this book from serious journals of history. But Harding's *Boulanger* comes from a reputable old-line publishing house, which touts it as "an authoritative biography," as "a lively, yet scholarly account of the life of the original man on a white horse." Let us overlook that word "yet" and its suggestion that "scholarly" and "lively" are incompatible, and let us even concede that the "white horse" phrase is a figure of speech, inasmuch as Boulanger's black horse is justly famed. What then must we do? Responsibility to even the minimum scholarly tenets of the historical profession requires that note be taken of some of the work's serious flaws.

The total absence of any internal documentation must be deplored. There is not, in this "scholarly" work, a single note citing the author's source of information. He tells of having got from Boulanger's grandson access to the family archives, which yielded "much unpublished material which has been incorporated in this book." There is no way of telling what Harding used from the Boulanger papers and where he used it. There is also a four-page appended bibliography that contains a strange, and unclassified, mixture of primary and secondary items, of ferociously *tendancés* tracts and scholarly studies. The text of the biography itself is liberally sprinkled—without a clue as to their basis—with conjecture as to motives and feelings and with (imaginary?) direct dialogue.

Further, a claim to objectivity with refer-

ence to Boulanger's career cannot be accepted from an author whose total bias against the Third Republic is implicit throughout the book. Concerning prominent Republican figures of the era, unrelievedly lurid and snide characterizations—spiced with elaborate gutter anecdotes—abound. Also there seems to be no institutional or political arrangement or practice of the maligned *Troisième* that is not held up to ridicule. While there can be no argument *ipso facto* with an attempt to revise the generally held unfavorable image of Boulanger, Harding's sympathetic image lacks credibility owing to the warped context in which the general is presented. Even though this biography makes no pose of being in the "life-and-times" genre, elementary decency demands serious effort at judicious and balanced treatment of other actors in the drama and of the setting in which they moved.

Harding is not a historian, and the absence of historical perspective that mars his life of Boulanger demonstrates the hazards that await the amateur when he treats one small segment in a complicated story—in this case, the story of the survival and consolidation of the Third Republic. No serious student of French history denies the sordidness, the shockingly unethical affairs that marked the 1880s in French public life. But with Harding and his smears of Boulanger's opponents these scandals are not adequately fitted into the Third Republic's ongoing development in which these very opponents, unsavory as many were, may nevertheless be seen as having made a genuine contribution to French democracy—to the point, even, where a reputable historian such as Georges Duveau could wish that "the German Social Democrats had shown with reference to Hitler a little of the energy that the Floquets and the Constans used to contain the Boulangist flood."

Any analysis of Boulanger must sooner or later come to revolve around the celebrated episode of the coup-that-wasn't and must take into account both how propitious in fact the moment was on January 27, 1889, and why the general behaved as he did. Harding with great dramatic detail builds up a scene of mob enthusiasm with strident voices urging a march on the Elysée. He portrays Boulanger as resisting the summons because to act in an overt

coup went counter to the habits of discipline and obedience engrained in him as a soldier and because of his dedication to legality. Harding's alleged "revision" consists of thus minimizing Boulanger's infatuation with his mistress as the cause of his inaction. But Frederic Seager (see David B. Ralston's review of *The Boulanger Affair*, *AHR*, 75 [1970]: 1138–39) has already shattered this "romantic" version of the tale. Seager, moreover, found no evidence of appeals for going "To the Elysée!" He states categorically that "the source of the legend of a *coup manqué* cannot be traced to contemporary accounts" (p. 204). Surely Harding, before he asks for his study to be taken seriously, should have come to grips with Seager's well-documented contention, if with nothing else.

JEAN T. JOUGHIN

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H. L. WESSELING. *Soldaat en krijger: Franse opvattingen over leger en oorlog, 1905–1914*. (Speculum Historiale, Number 4.) Assen: Van Gorcum & Comp. N.V.; Dr. H. J. Prakke & H. M. G. Prakke. 1969. Pp. 284. 29 gls.

The author of this balanced and intelligent book confesses that he chose his subject out of a feeling of astonishment. Astonishment at the fact that in the ten years before 1914 French writers and publicists of a much wider circle than just the more traditional rightist and conservative intelligentsia and for a larger public than just the small ordinary reactionary groups started to praise the maintenance and actual use of the French army and to idealize war in general in such a way that the author rightly speaks of a "serious delusion." To describe war, for example, as a "poème indicible du sang et de beauté," as was done by E. Psichari in 1908, requires a great deal of understanding.

The first three chapters try to paint the background against which this idealization of army and war has to be set. In chapter 1 the author sketches the rather ambiguous and awkward position of the French army during the first thirty years of the Third Republic: on the one hand accepted as the inevitable and necessary means by which a defeated nation regained its position of power in the world, on the other hand distrusted and rejected by the republican bourgeoisie, an idealistic intelligent-

sia, and the radical socialists out of antimilitaristic and pacifistic ideals and progressive political aims. From 1900 onward, however, the author discerns a general change of mood (ch. 2) that made acceptance of the French army easy, and former differences seemed to be smoothed over. A kind of "mal du siècle," a feeling of uncertainty and insufficiency, caused a strong reaction against what was seen to be "decadent" and "weak." A younger generation, of bourgeois and labor-class origin, seemed to be exhilarated by the great colonial-imperialistic successes of France and the impressive French victories in sports events and feats of aviation. And finally the international political crises of Tangier and Agadir (ch. 3) seemed to move public opinion in France toward real sympathy for a strong French army and toward a firm interest in theories that idealized war, without, of course, ever being as fully absorbed or as intellectually stimulated by such thoughts as were the more consistent and expressive interpreters of this trend of feeling.

Perhaps one could criticize this rather long introductory part of the book for its somewhat impressionistic approach. Whether the antimilitaristic and pacifist tendencies died, as the author seems to imply by remaining silent about them for the period after 1900, remains doubtful. Whether the mood of the "mal du siècle" was as serious as the author, by an excellent choice of quotations and references, wants to suggest is still open for discussion. Whether Tangier and Agadir were really stronger stimuli for the change of mood in France than some earlier international crises is a question that can also be put with some justification. On the other hand, one is grateful that the author avoids easy monocausality and simple determinism; rather, he attempts to describe shifts of moods embedded in a network of feelings and situations that taken individually did not necessarily cause such change nor inevitably result in a general war.

The main part of the book tends also to be an attempt at understanding by description rather than explaining by analysis. It is a systematic and well-ordered demonstration of the kind of expressed feelings and mentalities, reasonings and argumentations that glorified the army as a real school for the nation (ch. 4) and that idealized war as a blessing and purifica-

tion for that nation (ch. 5). The author offers no quantitative study measuring the change of public opinion—he refers with due praise to Eugen Weber's *The National Revival in France, 1905–1914* (1959) to which this study could be considered to be complementary—but instead he gives a qualitative description of a climate of opinion, carried forward and supported by such well-known and important writers as Péguy, Barrès, Brunetière, Massis, or Psichari.

The last paragraph of the last chapter describes the argument "la guerre pour la guerre" as the culmination of all war idealization in its most irrational style and as the perfection of the human necessity of "vivre dangereusement" (Emile Faguet). Such thoughts in their most consequential radicality are most certainly of the utmost importance to describe and to understand because they did not die after the First World War and were certainly not the monopoly of French prewar ideology.

I. SCHÖFFER

University of Leyden

ERIKA SPIVAKOVSKY. *Son of the Alhambra: Don Diego Hurtado de Mendoza, 1504–1575*. Austin: University of Texas Press. 1970. Pp. xvi, 450. \$10.00.

Not since the publication thirty years ago of Angel González Palencia's *Vida y obras de Don Diego Hurtado de Mendoza* (and for the first time in English) has there appeared a full-length biography of this important Spanish Renaissance humanist, philosopher, poet, historian, soldier, diplomat, and governor. Mrs. Spivakovsky's sympathetic treatment of Mendoza's active career does not stray far from Don Angel's distinguished work, but she does add a dimension to sixteenth-century history in her perceptive interpretation of Mendoza's role in Habsburg diplomacy.

Don Diego was a complex and sensitive personality, more responsive to the currents of thought transforming Europe in the early sixteenth century than were most servants of the Imperial Spanish crown. Yet he was staunchly traditional in his medieval conceptions of service and honor. His aggressive promotion of Charles V's interests in Italy during his ambassadorships in Venice (1539–44), Trent

(1545-46), and Rome (1547-52) and his vigorous governorship of Siena (1547-52)—resulting in repeated confrontations with the pugnacious Pope Paul III and the eventual revolt of the Sienese—is vividly described by Mrs. Spivakovsky. Much less space is devoted to Mendoza's writings. This may be understandable in view of the anomalous circumstances of their composition and Mendoza's strange reluctance to claim any of his literary creations, but, it is nevertheless unfortunate that Mrs. Spivakovsky did not see fit to give more than two pages to Mendoza's famous *Guerra de Granada* and only the briefest mention of his many poems. From her previous articles we know that Mrs. Spivakovsky attributes the *Lazarillo de Tormes* to Mendoza, but she says almost nothing here about this literary classic in the context of Don Diego's public service and makes no attempt to summarize her conclusions concerning its authorship. She does affirm, however, that Mendoza's intellectual bent was in part responsible for his failings as a man of affairs.

As a study of Mendoza's diplomatic career this is a valuable addition to a fuller understanding of the man and his times. Many insights into the tempestuous struggles among Habsburg, Valois, Farnese, and Medici are revealed. The author's documentation is extensive, especially from the archives at Simancas, Florence, and Madrid, but a rather jaundiced picture of Spanish-French and Spanish-papal relations results from the almost total neglect of French and papal sources. This is doubly surprising since the French and Vatican archives are among the richest and most accessible for this period. A further dimension would be added to Mendoza's portrait and the Habsburg-Valois rivalry would certainly appear more clearly if the observations and opinions of the French ambassadors were used along with those of Mendoza and his correspondents. All of which suggests that even this fine book is not the last word either on Mendoza or on the intricacies of power politics in the sixteenth century.

DE LAMAR JENSEN

Brigham Young University

ORLANDO RIBEIRO. *A evolução agrária no Portugal mediterrâneo: Notícia e comentário de uma obra de Albert Silbert*. (Chorographia, Série

Histórica.) Lisbon: Centro de Estudos Geográficos, Universidade de Lisboa. 1970. Pp. 226, 5 maps.

Orlando Ribeiro's little book is a commentary on and a critical evaluation of the massive two-volume (1,216-page) work of Albert Silbert, *Le Portugal Méditerranéen à la fin de l'ancien régime, XVIII^e—début du XIX^e siècle. Contribution à l'histoire agraire comparée* (Paris, 1966). Silbert's work deals with a region comprising the province of Alentejo and Lower Beira (*Beira-Baixa*), which together total about one-third of the whole country. This region has several distinctive geographical features, although I have some reservations about the division of Portugal into "Atlantic" and "Mediterranean" regions when the latter are, in fact, situated a long way from the sea of that name. Silbert's point of departure was the manuscript "Dicionário Geográfico" drawn up by order of Pombal after the disastrous earthquake of November 1, 1755, in order to try to ascertain the extent of the damage on a country-wide basis. As the questions were addressed to the parish priests, the replies are of unequal value, but many of them contain reliable statistics and statements concerning the local population and economy. Silbert also used a wide range of archival sources (many of them untapped before) and the relatively limited amount of printed material on Portuguese rural history that was available, supplemented, of course, by lengthy visits to the area. He was thus able to produce a work in the best tradition of the *Annales* school, a work densely documented from primary sources, which is essential reading for anyone concerned with Portugal in the period 1750-1850, and to which (as Ribeiro stresses) this monograph is a guide and commentary, rather than an exhaustive summary.

As the leading Portuguese historical geographer (who himself studied the same region over thirty years ago) Ribeiro is uniquely fitted to explain and evaluate the importance of Silbert's work, which Ribeiro does clearly, concisely, and fairly. Historians who cannot read Portuguese will be grateful for the twenty-five-page review in French that is reprinted at the end; but Ribeiro acknowledges that it is impossible to summarize Silbert's book adequately in this

length. One of the most interesting points that emerges is the persistence of various forms of collective agrarianism in this region. Ribeiro argues cogently that they probably derive from very remote (perhaps pre-Roman) times, whereas Silbert considers them to be of relatively recent origin. Forms of land tenure and of the rotation of crops varied widely, and both authors demonstrate that the large landed estates of the Alentejo differed greatly from the latifundia of Spain and southern Italy with which they have often been compared. The social structure of the towns and countryside is analyzed in meticulous detail. Agricultural, economic, and social historians will find Orlando Ribeiro's monograph an appetizing hors d'oeuvre to Silbert's very substantial banquet.

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J. GÉRARD-LIBOIS and JOSÉ GOTOVITCH. *L'an 40: La Belgique occupée*. Brussels: CRISP. [1971.] Pp. 517. 480 fr. B.

The central position of the authors of *L'an 40: La Belgique occupée* is that the German conquest and occupation of their country in 1940 aggravated all of the problems of Belgian society and that the issues faced in the first year of the subjugation continued to be the major issues for the remainder of the war. To make clear what the problems of Belgium were in 1940 the authors have an introductory section in which they sketch in rapid fashion the question of neutrality, the Flemish-Walloon controversy, the multiple-party system, and the military predicament of a small state.

After the "war of eighteen days," the situation within the country became ever more complex because of a variety of issues. First, there was the "royal question," which revolved around the decisions of the king to be commander-in-chief of the armies in the field rather than to remain at the head of his civilian government, to call for a surrender rather than to fight on, to remain in the country and to share the fate of his people rather than to go abroad and lead a resistance government, and to seek concessions from the occupier for his people. Second, there was the knotty question of organizing an autonomous administration

under German military authority and the making of inevitable compromises to keep the "Belgian" administration intact. There was the matter of getting the economy functioning in order to give people employment, even though it was known that much of the industrial production would go to Germany and that the Berlin clearing account would be used to avoid payment in full at market prices. Last, there was "la faim," which seems to have been the major catalyst in turning the people against the occupier.

All of these questions, and many more, are treated with competence and fairness. Yet the story fails to distinguish adequately between the important and the insignificant, contains such a vast amount of detail that a question may be obscured, and often leaves the reader wishing that some idea of the sequel were included in the narrative. Furthermore, the text is not easy reading. In fact, it seems to consist at times of disintegrated notes that were never reduced to a comprehensible form. The volume will, however, find a place in the literature of World War II, for it has a great reference value, is reliable, and has an excellent and extensive bibliography.

SHEPARD B. CLOUGH
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FINN GAD. *The History of Greenland*. Volume 1, *Earliest Times to 1700*. Translated from the Danish by ERNST DUPONT. Montreal: McGill-Queen's University Press. 1971. Pp. xiii, 350. \$16.50.

Scholars interested in the complex history of Greenland must not only use scant historical sources but rely upon recent archeological and anthropological information found scattered through relatively unfamiliar journals and written in Scandinavian languages with which they are often not familiar. It is thus particularly helpful to find here a brilliant synthesis of such materials written in English, which will certainly remain definitive for years to come.

Its author, Finn Gad, who has adapted this volume from his recent Danish work, which contains even fuller notes, deals with three major themes covering Greenland's history down to 1700 A.D. The first concerns itself with

pre-Eskimo and Eskimo migrations into Greenland from the Northeast, which began as early as 1600 B.C. and continued with certain breaks until the more recent neo-Eskimo colonization of the island from 1200 to 1700 A.D. His second theme deals with Norse settlement from the late tenth century down to its end during the late fifteenth century. His third theme concerns the rediscovery of Greenland and its Eskimo inhabitants by Europeans during the sixteenth and seventeenth centuries. Throughout, in order to explain the nature of Norse and Eskimo economy, society, and culture, the author makes use of a wealth of information derived from archeological and other research that helps illuminate many problems of Greenland's early history perplexing to scholars.

A few of his major points deserve emphasis. First of all he shows that Norse settlers arrived in Greenland during a period when it contained almost no native inhabitants, when it possessed a climate that allowed sheep and cattle to flourish, and when Norse settlers could exploit arctic resources, derived from narwhal and walrus, which were in great demand in Western Europe. Their settlements thus remained prosperous down to the thirteenth century. Only after 1200 did these Norse come in contact with neo-Eskimos, who possessed an arctic culture originating in Alaska and who were advancing down Greenland's east coast.

At first these contacts were peaceful ones and only grew hostile in the mid-fourteenth century when Eskimo pressures may have forced the abandoning of the northern Norse settlements. Whatever caused this abandonment it proved fatal to the colonists by cutting them off from those arctic products that made outside contacts with Europe profitable. Nevertheless, irregular connections with Europe were maintained for more than a century until the weakened survivors died out or were absorbed into a neo-Eskimo population that spread until it occupied the island's entire coastland.

The author believes that climatic change to colder temperatures played a role in all this by making animal husbandry increasingly difficult, by multiplying icebergs (which discouraged European mariners), and by producing a more arctic climate (which was particularly attuned to Eskimo culture). At any rate, it was a new and different Eskimo-inhabited Greenland that Eu-

ropean whalers and explorers encountered during the sixteenth and seventeenth centuries.

ARCHIBALD R. LEWIS

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GUNNAR QVIST. *Fredrika Bremer och kvinnans emancipation: Opinionshistoriska studier*. [Fredrika Bremer and Women's Emancipation: Studies in the History of Opinions]. (Kvinnohistoriskt Arkiv, Number 8. Scandinavian University Books.) Göteborg: Akademiförlaget. 1969. Pp. viii, 296. 36 S. kr.

Gunnar Qvist's book is one of those, part historical and part literary, that at worst fall irretrievably between two stools, and at their best produce new perspectives for historians and literary scholars alike. It can be firmly placed in the latter category. It contains a wealth of clear and well-documented material on the changing position of women in Sweden in the nineteenth century, while tracing the evolution of Fredrika Bremer's own views on the subject, principally from the beginning of the 1830s to the publication of her major novel *Hertha* in 1856.

It has long been held that *Hertha* was instrumental in bringing about reforms concerning the legal status of women, in particular the provisions of a government bill automatically allowing them to attain majority without resort to a court of law. Gunnar Qvist, however, demonstrates that Fredrika Bremer's view had little or no effect on this bill, the main lines of which had been decided before the publication of *Hertha*. The great debate resulting from the novel was concerned more with the broader perspective of Bremer's thought, in particular her semimystical idea that woman, being the younger creation, was closer to God and therefore in a special position to bring about an ennobling of the world. It was this utopian perspective that was at the center of the controversy; in this idea Brenner was alone, deserted even by those who supported her more practical demands.

Gunnar Qvist works methodically, giving first a general historical review of the decades into which he divides Bremer's work, then examining the works themselves and comparing their implications with what she says in her private letters, and finally taking a look at the reactions of the critics. After the *Hertha* debate

and a summary of her later views, it is, however, surprising to find a chapter on Bremer and the slave question in America. However interesting, this is not strictly speaking relevant to the rest of the book, as is perhaps implicitly accepted by the author, for he makes no reference to this chapter in his English summary. This summary is adequate, though one wishes the language had been checked by a competent English scholar.

W. GLYN JONES
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JOHANNES SZIBORSKY. *Die Germanisierung der Mark Brandenburg in der märkischen Geschichtsschreibung des 16. Jahrhunderts unter dem Einfluss von Humanismus und Reformation*. Breslau: [the author.] 1969. Pp. 272.

This book is a doctoral dissertation completed in 1969 at the University of Giessen. As a result of World War II Sziborsky, who was born in Breslau in 1929, was forced to leave that area, which was to become part of Poland, and he moved to Westphalia in West Germany. Here he completed his university education, which included studies at Münster and Düsseldorf. As with most German doctoral dissertations the documentation for this study is impressive. The text contains 206 type-written pages and added to this are some 28 pages of notes that contain 626 citations. These are all placed at the back of the book following the text.

The text is divided into four parts. First, there is the unnumbered *Aufgabe* or statement of the problem. Here the author clarifies the use of the term *Germanisierung*, which might be translated Germanization. This is a literary study, not a political history of Brandenburg. As a result Sziborsky assures the reader that the term Germanization must not be associated with the era of National Socialism or with earlier nationalism. From this portion he then turns to the introduction, which is an examination of medieval chronicles relating to the Germanization of Brandenburg. These include bishops' and princely records or chronicles as well as accounts of the Mark itself. From this introduction the author then turns to the main part where he examines the works of a number of writers such as Georg Sabinus, Ernst Brotuff, Wolfgang Jobst, and others. In the fourth and

last part (numbered three in the book since the *Aufgabe* receives no numeral), Sziborsky treats modern research.

For the specialist in the German language and for the student of literary history this is a valuable study. It is easy to read and well documented, and the format and size make it easy to handle. American graduate schools might well consider changing the size and form of American doctoral dissertations and copy some of the features of those in German universities. The $5\frac{1}{2} \times 8$ size, even though paper bound, is certainly easier to handle and to shelve than the traditional $8\frac{1}{2} \times 11$ American dissertations. Also, the *Aufgabe* in this particular book presents the problem more clearly than the usual preface, and the brief autobiographical sketch of the doctoral candidate can be very interesting.

R. N. CROSSLEY
St. Olaf College

URBAN PIERIUS. *Geschichte der kursächsischen Kirchen- und Schulreformation* (MS germ. quart. 91 der Staatsbibliothek Preussischer Kulturbesitz). Edited and with an introduction by THOMAS KLEIN. Marburg: N. G. Elwert Verlag. 1970. Pp. 718. DM 56.

In the last third of the sixteenth century, Electoral Saxony, the homeland of the Reformation, changed its religious course several times. Although Elector August had sternly suppressed all crypto-Calvinist tendencies and had imposed the Formula of Concord as the confessional norm, his son, Elector Christian (1586–91), accepted the Reformed position and, with the help of his most prominent counselors, Nikolaus Krell and Andreas Paull, gradually opened Saxony to Calvinist influences. Professors and pastors no longer had to sign the Formula of Concord, and reforms along Calvinist lines were initiated: exorcism was eliminated, pictures were removed from churches, liturgy and ceremonies were simplified, and the christological and sacramental definitions of the Formula of Concord were rejected. These changes along Reformed lines have been called a Second Reformation. After all, as historians have already distinguished a Protestant Reformation, a Magisterial Reformation, a Catholic Reformation, a Radical Reformation, and a Third Reformation, we

might as well add a Second Reformation. When Elector Christian died in 1591 the strictly Lutheran faction again seized control of the government. All Calvinist changes were immediately eliminated and the orthodox Lutheran position restored. In a fit of vengeance the most prominent Calvinist politicians were thrown into jail and Krell was executed in 1601 after ten years of imprisonment.

As superintendent at Dresden and afterward as professor of theology and general superintendent at Wittenberg, Urban Pierius (Urban Birnbaum, born in 1546 at Schwedt in Uckermark, died in 1616 at Bremen) was deeply involved in the Calvinist reforms. After the catastrophe of 1591 he occupied high positions in the Calvinist churches at Heidelberg, the Upper Palatinate, and in Bremen. It was at Bremen in or about 1608 that he wrote his *History of the Reformation of Church and School in Electoral Saxony* (covering the years from 1586 to 1591) in an effort to refute the violent accusations and calumnies raised against him and other Reformed theologians by Philipp Nicolai and other Lutheran authors. Pierius's *History* is based both on personal recollections and a considerable number of documents that had been saved before the disaster of 1591. Written in an unusually lively German style, the work is valuable because it presents a great amount of concrete information both on individuals and events, such as the characters and views of various powerful theologians and superintendents, hitherto unknown sermons held by court preachers before the Elector at Dresden, the effectiveness of censorship, or conditions at the university of Wittenberg. Although rich in factual and colorful detail, Pierius's history is strongly biased in favor of the Calvinists. And yet Pierius was such a shrewd observer and eloquent writer that one cannot help being fascinated by his book. In any case, it offers so many new and intimate facts that it is indispensable to our understanding of the fateful events in Saxony during the years from 1586 to 1591.

Lost for 250 years, the manuscript was discovered in 1962 in the collection of the former Prussian State Library by Thomas Klein, who had previously written a study of Saxony's Second Reformation. Klein has edited a slightly shortened version of the bulky manuscript,

adding a fine biographical study of Pierius and an extensive bibliography. Historians of sixteenth-century Protestantism will appreciate the publication of this unusually informative manuscript.

CLAUS-PETER CLASEN
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EDA SAGARRA. *Tradition and Revolution: German Literature and Society, 1830-1890.* (Literature and Society.) New York: Basic Books. 1971. Pp. 348. \$8.95.

Few literatures provide a richer field for investigating the transition from a traditionalistic to a modern social order than nineteenth-century German literature. For the few Western nations which negotiated that transition more smoothly and successfully than Germany did, the documentation of the problems posed by modernization, problems artistic and intellectual as well as political and social, is not so searching, intensive, or dense as for nineteenth-century Germany. There the hopes and expectations, the apprehensions and disappointments, the tensions and turmoil involved in modernization came to the fore time and again at the highest creative level. This creative encounter and its uniqueness are understandable when we consider that Germans were the first in a long line of peoples, in Europe and then throughout the world, to confront modernization as a threatening challenge from without, rather than as an urge born from within. The modern had to be assimilated, selectively and piecemeal, in Germany—grafted on to a system unprepared either to accept or reject it. How leading German writers, during each of Germany's phases of development from the Biedermeier period to Bismarck, came to grips with this situation is the theme of *Tradition and Revolution*.

The result is one of the few comprehensive and comprehensible works on the subject in English. The author's blend of social history and literary interpretation enables the reader at all times to see the creative process not only as a passive reflection of reality, but also as an original response to it. From her discussions of individual writers and their works, many familiar to English-speaking readers, some not, we

learn in each case what the landscape of the writer's vision was, how it came into being, wherein it accurately reproduced reality and wherein it transmuted reality, and what the writer's personal and artistic attitudes toward his material were. True, this book lacks the sociological acumen of a Karl Mannheim or an Ernst Kohn-Bramsted, the interpretive powers of a J. P. Stern or a Hans Mayer, and the philosophical and polemical verve of a George Lukács or a Lucien Goldmann. Still, *Tradition and Revolution* provides both a better understanding of nineteenth-century German history and a valuable contribution to our understanding and implementation of literature as a useful and unique source of historical documentation.

ROBERT ANCHOR

University of Southern California

JOHN R. GILLIS. *The Prussian Bureaucracy in Crisis, 1840-1860: Origins of an Administrative Ethos*. Stanford: Stanford University Press. 1971. Pp. xvi, 269. \$8.75.

German historians and sociologists have long been concerned with the role played by the bureaucracy in Prussia's historical development. Professor Gillis's book directs our attention to the two decades that bracketed the revolution of 1848. He argues that during these critical years conflicts within the bureaucracy combined with profound social and political changes in Prussia to produce a new "administrative ethos," one that forced the bureaucracy to assume a passive and "neutral" posture, allowing it to become a tool of authoritarian governments for the remainder of the century.

During the early decades of the nineteenth century the Prussian bureaucracy was inspired by a spirit of corporatism; defined as a *Beamtenstand* by the Prussian Code of 1794, it was, as Otto Camphausen described it, an "aristocracy of experts." By the 1840s, however, this corporate tradition was breaking under the stress of modernization, which had necessitated the introduction of a younger group of university-trained officials who soon grew frustrated by the intense competition for positions, poor pay, and a seniority system that protected older officials and slowed promotion based on merit. These younger officials gradually abandoned

the corporate image of the profession in favor of a moderately liberal one that demanded a fuller expression of civil liberties. Senior officials, on the other hand, responded to this pressure from below by imposing more difficult examinations for entrance into the civil service, by resorting to secret personnel files (*Konduitenlisten*) for blocking promotions, and by instituting a tighter disciplinary code. The revolution of 1848 brought this conflict into the open. Some officials wrote for liberal journals or became active in the liberal political organizations spawned by the revolution; others, especially older officials, joined conservative groups. These activities resulted in pressure for political conformity from both sides. First the liberal governments and then their conservative successors were willing to take disciplinary action against bureaucrats who endangered their programs. After Olmütz, the Manteuffel regime undertook to eliminate opposition from within the bureaucracy. In 1854 the *Konduitenlisten* were reintroduced, and politics became a prime consideration in promotion. Thus the failure of the revolution and the pressure of the reaction conspired to promote political obedience within the bureaucracy. Moreover, increasing professionalism fostered the expedient doctrine of political neutrality and with it, greater conformity.

Gillis's study, which draws heavily upon archival materials available in West Germany, offers significant insight into the Prussian bureaucracy during an important epoch. His last chapter, a description of the social composition of the bureaucracy from the 1850s, is especially helpful for the information it presents on the new governing class that emerged on the eve of German unification.

ROBERT M. BERDAHL

University of Oregon

WILHELM KLUTENTRETER. *Die Rheinische Zeitung von 1842/43 in der politischen und geistigen Bewegung des Vormärz*. Volume 1; volume 2, *Dokumente*. (Dortmunder Beiträge zur Zeitungsforschung, Number 10, parts 1 and 2.) [Dortmund: Verlag Fr. Wilh. Ruhfus.] 1966; 1967. Pp. 171; 173-266.

This history of *Die Rheinische Zeitung*, a paper that was confidently launched to match other leading journals in format, circulation,

and influence, rests on a thorough knowledge of the political and intellectual milieu of the 1840s, notably in the Prussian Rhine Province. Part 2 (*Dokumente*) includes biographical sketches of the contributors together with a listing of the articles that each wrote, though complete certainty was difficult in some instances because all items appeared anonymously. The statutes of the newspaper company, financial statements, the publication contract, and a number of contemporary appraisals of the nature and impact of the *Zeitung* appear among the documents.

Originally favored by Prussia and financed by loyal bourgeois stockholders, the paper early came under the editorial control of a younger generation of Young Hegelian radicals. Karl Marx was influential from the first from behind the scenes and ultimately became its chief editor. This study should help to dispel the impression that the *Zeitung* experienced a distinctly radical shift under his disciplined editorship. Through a special listing of all articles dealing with press questions the author stresses the role of the paper in the discussion of the current lively issue of freedom of the press. A sample of the less favorable contemporary evaluations of the paper, other than those that came from official sources, might have been included. The journal was perhaps more widely damned than praised and may owe its immortality to the fact that it was suppressed because the "liberalized" Prussian censorship regulations judged a paper by its tendency (*Tendenz*).

OSCAR J. HAMMEN
University of Montana

EBERHARD KOLB. *Die Kriegausbruch 1870: Politische Entscheidungsprozesse und Verantwortlichkeiten in der Julikrise 1870*. Göttingen: Vandenhoeck & Ruprecht. 1970. Pp. 150. DM 15.80.

The controversy over responsibilities for World War I has overshadowed another similar debate that has been going on even longer. It concerns the origin of the Franco-Prussian War of 1870-71. Was it France and Napoleon III-Ollivier-Gramont who caused that war, was it Prussia and Bismarck, or was the "war guilt" shared by both sides?

Dr. Kolb acquits Bismarck of having planned the collision and places the blame for the war on the French leaders. There is nothing in the sources, he claims, to indicate that Bismarck wished to maneuver France into a war when he urged that Prince Leopold of Hohenzollern-Sigmaringen accept the candidacy to the Spanish throne. Kolb sees the key event that touched off the crisis in the warning given by the French government that France would never tolerate a Hohenzollern prince on the throne of Spain. This statement, made before a wildly cheering chamber of deputies, roused French emotions to fever pitch and deprived the government of its freedom of action.

When Prince Leopold abandoned his candidacy, Kolb continues his argument, the French leadership could not content itself with this success, but felt compelled for domestic reasons to demonstrate even more clearly France's preponderance over Prussia. This it did by asking King William I of Prussia for a pledge never to allow any Hohenzollern to ascend the Spanish throne. The request was denied, with the known consequences. Given the determination in Paris for a showdown, Kolb concludes, the Ems dispatch played but a minor role in precipitating the crisis.

Kolb argues his case with a great deal of ingenuity, but his analysis fails to convince. Apart from the fact that he passes over material that shows Bismarck's conduct in a much more unfavorable light, the available evidence is ambiguous and lends itself to various interpretations. (Josef Becker, in an essay published in *Historische Zeitschrift* [212 (June 1971): 529-607], examines many of the same sources that Kolb used but arrives at conclusions refuting Kolb's thesis.)

More important, Kolb all but excludes domestic considerations from his investigation. Yet there is a good deal of evidence suggesting that the completion of German unification, if it was to be achieved from above, did not permit any further delay and could be accomplished only amidst a national crisis, such as a war with France. Pressures from business circles, the prospect of a reassertion of the liberal camp, and serious threats to the budgetary arrangements concerning the army most likely also affected the chancellor's actions. In any event these factors cannot simply be brushed

aside, as they are by Kolb. However, given the circumstantial nature of the evidence, the debate will doubtless continue—without ever being settled conclusively.

ANDREAS DORPALEN
Ohio State University

WINFRIED SCHÜLER. *Der Bayreuther Kreis von seiner Entstehung bis zum Ausgang der Wilhelminischen Ära: Wagnerkult und Kulturreform im Geiste völkischer Weltanschauung.* (Neue Münstersche Beiträge zur Geschichtsforschung, Number 12.) Münster: Verlag Aschen-dorff. 1971. Pp. viii, 293. DM 48.

The so-called "conservative revolution" of the nineteenth and twentieth centuries attacked the modern forces expressed in the French and Industrial Revolutions and preached a return to an idealized, lost world. One of the most interesting but previously neglected examples of this movement in Germany was the Bayreuth Circle, a loosely structured group of enthusiasts who propagated both Richard Wagner's artistic and national-racial ideas. Winfried Schüler's book is the first critical history of the Bayreuth Circle and is a welcome addition to the literature on the growth of radical conservatism in Imperial Germany. It is based primarily on a thorough examination of hitherto unused archival material in the Richard Wagner Gedenkstätte in Bayreuth, in the Ludwig Schemann *Nachlass* in Freiburg, and on an examination of the literary organ of the Bayreuth Circle, the *Bayreuther Blätter*.

An opening section discusses the development of Wagner's thought and the varied manifestations of the cultural reform movement in Imperial Germany. Schüler then traces the historical background of the Bayreuth Circle and presents short biographies of its leading figures, such as Cosima Wagner, Hans von Wolzogen, and Houston Stewart Chamberlain, as well as a listing of lesser-known men. The third section discusses the *Weltanschauung* of Bayreuth. Wagner's criticism of contemporary theater rapidly developed into a criticism of society in general. The key word in his *Weltanschauung* became "regeneration," and he saw an intimate connection between artistic and cultural regeneration. Schüler indicates that Wagner's theories, especially those of his last writings, were rapidly dogmatized after the composer's death.

For Wagner's disciples Bayreuth became a symbol of longed for artistic and cultural regeneration. In true Romantic fashion the Bayreuth Circle saw art as better able to penetrate to a deeper reality than other modes of knowledge and as best expressing the strivings of the *Volk*. For Bayreuth, key elements of regeneration became racism and anti-Semitism, since the Jews were seen as the agents of modernity in all its forms. Beginning with Wagner's essay on the Jews in music, this racism received its ideological foundation in Gobineau's works and its development into an optimistic and influential viewpoint in Chamberlain's *Grundlagen des neunzehnten Jahrhunderts* (1899). Such racism also led to Bayreuth's championing of a Semitic-free "Germanic Christianity."

Thus, Bayreuth cultivated many of the ideological components that helped prepare the way for National Socialism. As Schüler points out, the esthetic drive is one of the great gifts of mankind, but it is unsuitable as a foundation for metaphysical theories and political doctrines.

DONALD E. THOMAS, JR.
Virginia Military Institute

HANS GEORG LEHMANN. *Die Agrarfrage in der Theorie und Praxis der deutschen und internationalen Sozialdemokratie: Vom Marxismus zum Revisionismus und Bolschewismus.* (Tübinger Studien zur Geschichte und Politik, Number 26.) Tübingen: J. C. B. Mohr (Paul Siebeck). 1970. Pp. xvi, 329. Cloth DM 58, paper DM 52.

The author of this seemingly specialized monograph adroitly draws out its many broader ramifications. Narrowly defined, his subject concerns the protracted debate within the German Social Democratic party over the adoption of an agrarian program in the decade 1890-1900. But Lehmann relates this internecine conflict to the broader evolution of the party toward electoral politics, the consequent need to attract new voters in a population still heavily composed of peasants, and the agonizing that resulted over what—in good conscience—the party might promise to a class Marx regarded as doomed. Reformist forces within the party gathered strength largely around this issue, and Bernstein's revisionism derived in no small part from the debate over

whether the law of concentration applied to agriculture. The reformist-inspired draft program was ultimately defeated in 1895 in a triumph of Kautskian orthodoxy, which mobilized strong rank-and-file opposition against any concessions to rural property owners. This triumph, however, left the party without any agrarian program and with a sterile indifference toward peasants that most of the other Second International parties obediently copied from the German *Musterpartei*. It remained for Lenin, who took some part in these West European controversies, to appreciate the full value of peasant support and find the right programmatic formula for winning it. Both revisionism and Leninism thus have important roots in the agrarian debates of 1890–1900.

Originally a 1966 Tübingen dissertation, this study utilizes all the appropriate sources, published and unpublished, and skillfully weaves together several levels of analysis—rank-and-file pressures, regional and national leadership conflicts, and international influences. Lehmann writes dispassionately and well, avoiding the temptation of social-science jargon. Despite the title, his treatment of non-German countries is very meager; essentially he writes SPD party history.

Perhaps because he does not seriously treat rural social conditions, Lehmann leaves the unfortunate impression that the SPD might have turned Lenin's trick in Germany if only it had discovered his magic formula, the "correct" agrarian program. Doubtless Lenin showed great political acumen in riding to power in Russia chiefly on a massive tidal wave of rural discontent, but surely he did not create the tidal wave himself. It did not exist in the West during this period, and no programmatic or tactical legerdemain could have produced it. Thus, the Nantes agrarian program of 1894 profited the French party scarcely more than the absence of such a program did the German, and the belated 1927 statement finally adopted by the SPD was equally barren in the result. No magic formula but only a certain cluster of social conditions can transform the peasantry from an immovable object into an irresistible force.

RICHARD N. HUNT
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URSULA FEIST and GÜNTER FEIST, editors: *Kunst und Künstler: Aus 32 Jahrgängen einer deutschen Kunstzeitschrift*. [Mainz:] Florian Kupferberg. 1971. Pp. 441. DM 36.

BETH IRWIN LEWIS. *George Grosz: Art and Politics in the Weimar Republic*. Madison: University of Wisconsin Press. 1971. Pp. xviii, 328. \$12.50.

From 1902 to 1933 the Berlin periodical *Kunst und Künstler* was an influential voice of modern art—especially of Impressionism—in Germany. Its contributors included critics, historians, and museum officials as well as artists and architects writing on their own work and on that of their peers. As significant as its exploration of modern art throughout the world and of its historical antecedents were the journal's critical discussions of the cultural policies of succeeding German governments—patronage, purchasing practices that often betrayed a dislike of the modern and foreign, appointments to the staffs of museums and academies, and attempts at censoring art. In a volume that is produced with the same distinguished taste that marked the original issues of *Kunst und Künstler*, Ursula and Günter Feist have brought together a selection of articles, reviews, and illustrations that, with their careful notes and bibliography, provide an excellent introduction to the journal. In an epilogue Günter Feist traces the course of *Kunst und Künstler* from its beginnings to its demise when Hitler came to power. Mr. Feist's interpretations show an impressive sensitivity to the past, but his conclusions are repeatedly hobbled by unhistorical Marxist assertions. He offers a knowledgeable analysis of the journal's mission and of the campaigns that it waged; but his admiration for its achievements is tempered by his distaste for its upper-middle-class liberalism and its opposition to social criticism and participation in the class struggle, "which was and remains a necessity of our age" (p. 399). Elsewhere an essay on Van Gogh by Karl Scheffler, who edited *Kunst und Künstler* for twenty-eight years, is judged "unacceptable" on political grounds; and similar pronouncements intrude throughout the essay and notes. No wonder Feist, who welcomes art that supports revolution but rejects art that supports the status quo, experiences some difficulty in coming to terms with a journal that consistently held

that the only judgment in art that mattered was whether it was good or bad.

In its defense of Impressionism *Kunst und Künstler* soon attained an impregnable intellectual position, though the public as a whole was by no means won over. The journal showed some reluctance to extend its sympathies to other modern directions—Expressionism, for instance, or to such more ephemeral movements as Dada. Some of their adherents, moreover, turned aggressively against Impressionism and its leading German representatives, who were among the journal's most valued contributors. Nevertheless Scheffler and his colleagues were sufficiently broad-minded to publish and discuss the work of the new men.

One of the radical artists who appeared in the pages of *Kunst und Künstler* and whom Scheffler defended against right-wing attacks was the painter and cartoonist George Grosz. Beth Irwin Lewis has written an intelligent, well-informed account of Grosz's "experience and work against the background of cultural and political life in Germany before 1933." In view of her judicious treatment of complex events it may be that the author's intentions were unduly modest. For example, disclaiming competence in handling the tools of psychohistory, she scarcely explores Grosz's psychological conflicts, though she recognizes their influence on his political attitudes as well as on his art. Here as elsewhere the important issues of his life are reported by Mrs. Lewis rather than analyzed. We might, in particular, have wished for a fuller discussion of the clash between esthetic independence and party discipline—the classic conflict of the politically engaged artist—which Grosz resolved in the early 1930s by turning away from a communism that he had come to see as basically identical with fascism. Within her self-imposed limits, however, Mrs. Lewis is successful.

Grosz's bitter drawings of officers, profiteers, and exhausted factory workers have long been valued as suggestive of the atmosphere in Berlin during and after the First World War. The pages of *Kunst und Künstler* open the view to another strand in the history of the Empire and the republic. The journal was a preceptor of the cultural attitudes of German—especially north-German—liberalism and was one of its

most representative spokesmen. It reflected the values of a group that was to suffer total eclipse but that possessed the merit—rare in times of frenetic nationalism—of feeling and acting as Europeans as much as Germans.

PETER PARET
Stanford University

Year Book XV. (Publications of the Leo Baeck Institute.) [London:] East and West Library for the Institute; distrib. by Leo Baeck Institute, New York. 1970. Pp. xviii, 326. \$9.50.

The Leo Baeck Institute is an organization dedicated to nostalgic research in the history of German Jewry. Since the number of Jews who still speak German with perfection is now shrinking rapidly, the exploration of their cultural past is almost akin to salvaging treasures from a sunken ship. The stories, raised from the bottom—cleaned, polished, and handled with loving care—are assembled into year-books, outfitted with bibliographies, indexes, and glossy photographs and given an inspiring theme, such as *Precursors of Integration—Defiance in Destruction*, the subtitle adorning the cover of volume 15.

Actually, the current yearbook is about people, most of them old friends well known to the authors and familiar to prospective readers. We meet the novelist Arnold Zweig as a disappointed emigrant in Palestine, the star of Curt Worman's essay "German Jews in Israel." The nineteenth-century banker Ludwig Bamberger and his contemporary, the publisher Leopold Sonnemann, are the centerpieces of Werner Mosse's article, "The Conflict of Liberalism and Nationalism and its Effect on German Jewry." Karl Marx is presented by Arthur Prinz in "New Perspectives" as a Jew, although some of these findings are based on papers that were previously discovered and analyzed by such investigators as Boris Nikolayevsky and Lewis Feuer. To make Marx a little more Jewish than he was, the author mistranslates Marx's reference to Disraeli as a *Stammgenosse* (one who is descended from the same group) as "our fellow Jew."

Twenty-seven pages are devoted to Ernst Loewenberg's memoir of the writer Jakob Loewenberg. Here we learn that "Loewenberg's childhood in a Westphalian village had

given him a deep love of nature," and we are reassured that "Loewenberg was never heard to make an unfriendly remark about Eastern Jews. They were familiar to him from his early childhood."

An East German researcher, Helmut Eschwege, is featured with a discussion of the resistance of German Jews against the Nazi regime, the "first treatment at any length of the role of Jews in the German resistance movement." Eschwege opens his report with lists of Jewish Social Democrats who died in concentration camps during the 1930s and works up to the small communist Jewish cell of Herbert Baum, which set fire to a Nazi propaganda exhibit in Berlin on May 18, 1942.

Five of the ten authors in this volume are more than seventy years old. The oldest of them, Selma Stern-Taeubler, "Grand Old Lady of German-Jewish Historiography," deals extensively with one of the older subjects, "The First Generation of Emancipated Jews," including especially Moses Mendelssohn. The youngest contributor, Rabbi Arthur Hertzberg, concludes the volume with a two-page reminiscence of Ernst Cassirer's last winter at Columbia University. It is a touching note. Here we see how much more can be done with a few sensitive strokes of the pen to portray a man's search for his soul than was managed in all the preceding footnote-laden articles put together.

RAUL HILBERG
University of Vermont

GEORGE W. F. HALLGARTEN. *Als die Schatten fielen: Erinnerungen vom Jahrhundertbeginn zur Jahrtausendwende*. [Berlin:] Ullstein. 1969. Pp. 366.

Historians familiar with George Hallgarten's works on imperialism, dictatorships, armaments races, and German National Socialism have been fully aware of his wide learning, sustained intellectual energy, readiness for scholarly debate, and continuous interest in understanding power in its many varieties and applications. He has now undertaken the venturesome and hazardous task of recounting the life story out of which his works emerged. Throughout these memoirs, from the recollections of his childhood and youth in an upper-bourgeois, assimilated Jewish family in Munich

before the First World War, through the narrative of the refugee's travels and searches, to the overly detailed descriptions of lecture tours in the 1960s, the personal characteristics of the man and the scholar appear repeatedly: an awareness of inherited social status, an admiration for and a dependency on his mother, a fascination with the structure and dynamics of power (Machiavelli, Clausewitz, and Max Weber have been his favorite social-scientific authors), a tendency toward difficulties in personal relationships, and a persistent longing, only partially fulfilled, for scholarly recognition and importance.

Although the substance of the book is often intriguing and the style is smooth and even captivating, its worth for students of twentieth-century history is more difficult to evaluate. Throughout his life Hallgarten has known persons of prominence, and some of notoriety, and the pages of his memoirs are sprinkled liberally with their names—the Thomas Mann family (especially Erika and Klaus), Ludwig Ganghofer, Heinrich Himmler, Hans Pfitzner, Erich Marcks, Hermann Oncken, Max and Alfred Weber, Eckert Kehr, Richard Graf Coudenhove-Kalergi, Carlo Mierendorf, and many more. For a few of these, Hallgarten's impressions and characterizations will offer an added perspective and occasionally a new detail for future biographers. But on many others, those he knew only in passing, his comments are cursory and inconsequential. He has added to the information contained in an earlier article concerning his childhood acquaintance, Heinrich Himmler, but his mixture of reminiscence and interpretation must itself be scrutinized carefully to distinguish the subjective from the informative elements. Despite Hallgarten's inclination to comment briefly on many personalities, that is not the aspect of these memoirs which may prove to be of historical interest. It is only when his experiences fit into a broad cultural context or into social-political movements that his personal account assumes more significance. In the early chapters many of the features of the social and cultural milieu in which he grew up come through with vividness and credibility. From the account of his participation in student activities in the 1920s, both in Heidelberg and Munich, one can perceive some of the reasons why the sympathizers with

the Weimar Republic, student socialists, academic republicans, and the advocates of international understanding and pacifism were not more effective. As cultural elitists and political neophytes, they talked ideas and remained socially insulated from the day-to-day realities of the world for which they were so concerned. To his credit Hallgarten does not hide these weaknesses, but he seldom explores their ramifications. It is a curious and revealing oversight, for example, that in his discussions of Weimar politics and elections he does not indicate how he voted or whether indeed he did vote.

Hallgarten has contributed his own chapters to the larger story of the vexations, anxieties, and the successes of the emigrants and exiles from a Germany that rejected them and that they could not fully comprehend. Every emigrant experience had its special perplexities, and Hallgarten's were the frequently frustrated goals of finding an appropriate position for himself and a publisher for his massive manuscript on imperialism. The publishers of this book could have enhanced its usefulness had they included an index in addition to the bibliography of Hallgarten's publications.

VERNON L. LIDTKE

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Probleme deutscher Zeitgeschichte. (Lund Studies in International History, Number 2. Scandinavian University Books.) [Stockholm:] Läromedelsförlagen. 1971. Pp. 266.

Probleme deutscher Zeitgeschichte is the second volume in a series of studies in "international history," a concept broadly understood by the publisher, the department of history at the University of Lund, Sweden, to include "not only foreign policy and international relations but also the history of different countries outside Scandinavia." The series encompasses a number of monographic works as well as collections of essays and shorter studies.

This volume falls into the latter category and consists of six chapters by various contributors on topics in twentieth-century German history: the German Monroe Doctrine in the Baltic Sea during the First World War; Raeder and German naval strategy; propaganda on the *Reichstag* fire; the Hossbach Memorandum;

Doenitz's government program in 1945; and Hitler's cultural policy after 1933.

One may, in good conscience, commend the authors in several respects. Their research is quite thorough, both in the secondary literature and the primary sources, notably the archives of the German Foreign Office and the *Bundesarchiv* in Koblenz. The essays are generally well written and the arguments clearly presented, and the translation into German is coherent and lucid.

Unfortunately these basic assets do not compensate for the serious deficiencies of the volume. In the first place, both collectively and individually, the essays reflect an almost antiquarian interest in Germany's recent past. Of the six, two are concerned with problems of naval history that convey a distinct impression of singular insignificance. Germany's so-called Monroe Doctrine during the First World War, for example, was certainly a matter of secondary, or even tertiary, importance at the time and ceased to have any relevance whatsoever when the German army fell apart in 1918. Similarly a study of Raeder's naval strategy and the sources of its inspiration does not add appreciably to our understanding of the more fundamental issues of twentieth-century German history. And even when the subject of discussion is a problem of more serious concern, such as the *Reichstag* fire, the author focuses on various contributions to the journalistic cacophony surrounding the crisis and not on the significance of the crisis itself.

Second, the collection as a whole betrays an unimaginative and almost sterile approach to some of the broader aspects of the historical discipline. In his essay on Doenitz, for example, Lennart Sjöstedt includes a somewhat self-righteous admonition on the dangers of contemporary history in which he contends that proximity in time makes it "generally more difficult to view the subject *objectively*" (my italics) and more likely that the author will "incline sentimentally in the one or another direction." An adequate rejoinder to this contention would transcend the limits of the present review, but for the moment it does not seem unreasonable to point out that Sjöstedt's argument is vulnerable to challenge on several grounds at least.

In conclusion one must necessarily hope that

this volume will not be representative of the Lund series either in terms of the scholarly issues it raises or the more philosophical sense of the historical discipline it projects.

RICHARD M. BERNSTEIN
Wellesley College

SHLONIO ARONSON. *Reinhard Heydrich und die Frühgeschichte von Gestapo und SD*. (Studien zur Zeitgeschichte.) Stuttgart: Deutsche Verlags-Anstalt. 1971. Pp. 339. DM 20.

Mr. Aronson's stated objective is to add to our understanding of how the ultimate character of the Nazi state emerged, how certain aspects of the ideology and certain elements of the Nazi movement prevailed, and how the basis of the "final solution" was laid. The vehicles are a short biography of Reinhard Heydrich and a detailed study of the formative years (1931-35) of the organizations he commanded, the Nazi Security Service (SD) and the Gestapo, the political police of the Third Reich.

In fulfilling his objectives Aronson has added refinement and further substantiation to the interpretations of Hans Buchheim and Friedrich Zipfel. His account of the emergence of the SD from the swirl of competing intelligence agencies is a pioneering effort. An important contribution is the thesis that competition between the Gestapo and the SD drove them to propose increasingly radical solutions to the "Jewish problem."

The biography of Heydrich confronts us with another, unheralded objective—a convincing effort to lay to rest the persistent rumor of Heydrich's Jewish ancestry. Aronson's portrayal of Heydrich is satisfying. The man who emerges is a real, but pitiable human being rather than a demonic superman. His role during the period studied is also reduced to more realistic proportions than that usually attributed to him.

The frequently told story of the struggle for control of the police is enhanced by Aronson's analysis of what produced the final product. Traditional German bureaucratic institutions are contrasted with Himmler's innovative SS-police-concentration camp model developed in Bavaria. Both added to the ill-defined and multifaceted character of the final system. The resultant flexibility and ambiguity defeated

Heinrich Himmler's opposition and aided in the transformation of the heterogeneous personnel of the Gestapo and SD into functionaries of mass murder.

Aronson has thoroughly tested the traditional eyewitness versions of SD and Gestapo history, which previous scholars have accepted as the best available information. For this, he interviewed a much wider range of surviving participants and drew upon the core of relevant archival material available to Western scholars.

Overly detailed organizational descriptions and almost forty biographical sketches confuse the narrative. Although the personality profiles were essential to Aronson's characterizations of Gestapo and SD men, much of the detail could have been relegated to appendixes. These characterizations and the explanations of how such men were lured into their future roles are highly plausible. The small elite sample on which they are based nevertheless points to the need for a more thorough quantitative study. Even after this has been done Aronson's account will probably remain unchallenged except on minor details or subtleties of interpretation, barring some very surprising revelations from East European archives.

GEORGE C. BROWDER
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Fredonia

JOHN H. BACKER. *Priming the German Economy: American Occupational Policies, 1945-1948*. Durham: Duke University Press. 1971. Pp. 212. \$6.75.

Almost a generation after the event, analysis of U.S. post-World War II occupation policy in Germany survives as at least a minor sport for both Americans and Germans as evidenced by John Gimbel's sober *American Occupation of Germany* (1968) and Caspar Schrenck-Notzing's far-out *Charakterwünsche* (1965). Where *Priming the German Economy* fits into the literature of the occupation is not easy to determine offhand. Although John H. Backer has done substantial research, it is not, strictly speaking, a history even of economic policy; and although Backer worked in the Economics Division, OMGUS, it is also not a memoir. It is, nevertheless, a valuable small book that future

writers on the occupation will do well not to ignore.

In the first three chapters Backer gives sound, if somewhat rambling, observations on U.S. economic policies in general and their execution, particularly the famous JCS 1067, its "disease and unrest" formula, and the level of industry plans. The subsequent chapters (4 through 6) are an extended essay on the U.S. export-import program and the U.S.-British Joint Export-Import Agency. There Backer is on his home ground. As a former official of the export-import program for Bavaria he writes from experience, but he has not neglected the documents, in this case the OMGUS records in the National Archives. The result is as lucid an account of one of the least comprehensible features of the occupation as is likely to be written.

Backer is one of the few writers on the occupation who have successfully resisted the subject's built-in temptations to blanketed, and hence often pointless, negative judgments. From time to time in fact he leans too far the other way, for instance, when he writes that JCS 1067 was "no impediment to the early efforts of the American Military Government toward the reconstruction of the German economy." While it was certainly not a practical impediment, especially since the German economy was already as badly off as it would have been under JCS 1067 stringently construed, it was a psychological impediment in that it created an apparent divergence between the stated U.S. policy and the manner of its execution.

Backer is also, in my opinion, a little too willing to equate good performance with general success. True, both the food and the related export-import programs for the Germans were enlightened and constructive, but the simple fact was that both were largely exercises in futility without currency reform and superfluous after currency reform was finally achieved two or three years later.

EARL F. ZIEMKE
University of Georgia

JOSEPH FREDERICK ZACEK. *Palacký: The Historian as Scholar and Nationalist*. (Studies in European History, Number 5.) The Hague: Mouton. 1970. Pp. xiv, 137. 28 gls.

In this slender volume the contributions of František Palacký to historiography receive their fullest appreciation in English since Francis Count Lützow wrote some seventy years ago. Those readers already acquainted with Joseph F. Zacek's articles on the nineteenth-century historian and public figure will find here more than a convenient compilation, and those not acquainted will be admirably introduced to the pre-eminent personality in the Czech national revival. An updated version of the author's doctoral dissertation, it is more elegantly written than most such endeavors.

Palacký's jousts with Habsburg censors, which prompted him vigorously to defend freedom of scholarly inquiry, exemplified the burdens of the liberal intellectual in pre-March Austria. His devastating rebuttals to German detractors of his monumental *History of the Czech Nation* (1836-67) established a model of combative yet objective scholarship in a national cause. His treatment of fifteenth-century Central Europe shattered hoary myths and elevated Hussitism to European and even transcendent importance. This pioneering role was imposed upon Palacký by the necessities of locating and assembling the pertinent historical sources and of creating upon that foundation the first scientific account and philosophical interpretation of Czech history.

The author views his monograph as "a partial, critical synthesis," the prelude to a full-scale biography that would draw intensively upon archival materials, but the work can stand on its own merits. It is not merely an essay in historiography and biography but also a revealing glance into a pivotal arena of awakening nationalisms. One wishes, however, that Professor Zacek had refrained from repetitively designating almost all citations of Czech scholarship since 1948 as "Marxist" or "Marxist-oriented." He could have made this obvious point more effectively in the preface or the bibliographical essay. And surely it is carrying respect for a dead hero too far when he asserts that Palacký's "broad modern historical program . . . remains the basis of contemporary Czechoslovak historiography." For better and for worse today's Czech historians have incorporated the problems, themes, and methods bequeathed them by Palacký into a different and more relevant perspective; and if Slovak histo-

rians also supposedly follow Palacký, then perhaps the author has interpreted the great historian's "program" so broadly that it acquires well-nigh universal validity.

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ANTON VANTUCH and L'UDOVÍT HOLOTÍK, editors. *Der österreichisch-ungarische Ausgleich 1867: Materialien (Referate und Diskussion) der internationalen Konferenz in Bratislava 28.8.–1.9. 1967.* (Slovenská Akadémia Vied, Historický Ústav.) Bratislava: Verlag der Slowakischen Akademie der Wissenschaften. 1971. Pp. 1076. 120 K.

The Habsburg monarchy died in 1918, but as yet no certifiable cause of death has been established. At first death was attributed to malignant nationalism aggravated by the First World War, but further dissection of the corpse cast doubt upon this conclusion. New evidence suggested that nationalism was not causal but part of wider processes of historical change that became compelling in the nineteenth century. This suggests that the insoluble nationality conflicts were themselves only a symptom of a systemic dysfunction brought on by the limited adaptability of the monarchy's sociopolitical structure to these processes. In the period between the two world wars research concentrated mainly on the political, diplomatic, legal, and cultural dimensions of the problem, while after World War II, Marxist-oriented historians in East Central Europe stressed long-run economic and social forces associated with the processes of modernization. The latter analysis was valuable but it remained separate until recent years. As a way of exploring the interconnection between the findings of Marxist and non-Marxist historiography, international meetings of historians were held in Budapest (1964), Bloomington, Indiana (1966), Bratislava (1967), and Vienna (1968)—to name the major ones. Each conference was held on the anniversary of what might be labeled a predisposing event, that is, an event that predisposed the Habsburg monarchy to disintegration. This volume contains the papers of the Bratislava Conference held in the summer of 1967 on the hundredth anniversary of the Compromise of 1867, which created the dual monarchy of Austria-Hungary.

The Bratislava Conference, which was sponsored by the Historical Institute of the Slovakian Academy of Sciences, brought together 140 Marxist and non-Marxist historians from 15 countries. The 42 published papers and 155 pages of discussion contain a wealth of analyses, data, bibliography, and research suggestions. On the whole the papers and discussions of this conference are free from the East-West division of earlier conferences. There are disagreements among both Marxist and non-Marxist historians, and methodological and conceptual differences tend to be of the kind about which historians generally argue. That is, they differ in their predilections for interpretations based on long-run factors as opposed to relativistic ones and the significance of that which happened as opposed to that which might have happened but did not. Progress toward integration of different lines of historical research, mutual jettisoning of outworn ideological baggage, and less apologetic stances by East Central European and Austrian historians—which are all, one hopes, irreversible trends—are attested by concluding remarks of several of the participants. For example, Jiří Kořalka, a Czech, remarks that "black-white" historical interpretations of the Habsburg monarchy have declined greatly, and he calls upon his fellow Czech historians to concern themselves with political and constitutional history more than they have in the past (p. 1041). Hans Lentze, an Austrian, calls on his compatriots to pay more attention to social and economic history (p. 1038).

The forty-two papers are grouped together under four headings according to their content. In introductory papers Josef Polišený (Prague) and Fran Zwitter (Ljubljana) maintain that the revolutions of 1848–49, which represent the beginning of the end for the Habsburg monarchy, are far more important than the Compromise of 1867, which they see as a last and negative echo of the revolution. R. A. Kann (Rutgers), on the other hand, sees little of the ideas or plans of 1848–49 in the Compromise of 1867, which he asserts was never intended to solve the nationalities problem but was meant to preserve the Habsburg monarchy and its great power position with the absolute minimum of social change necessary. Several of the participants took issue with Kann's conclu-

sion that the compromise ought to be judged "only according to the purpose of those who created it" (p. 25).

The first group of papers examines Austro-Hungarian foreign policy from 1866 to 1870, the implications of the Compromise of 1867 for the Eastern Question, the reaction of Austria-Hungary's small southeastern neighbors, as well as the attitudes of the respective foreign offices and major newspapers of the great powers toward the compromise. In general the essays show the connection between foreign policy and domestic structure and how, for different reasons, the reorganization brought about by the compromise accorded with the interests of all of the great powers.

The second group of papers deals with social, economic, and constitutional aspects of the compromise. Walter Goldinger (Vienna) and Hans Mommsen (Heidelberg) both stress the continuation of absolutism as a logical extension of the compromise. Mommsen attributes the insolubility of the nationalities question in Austria to the quasi-constitutional and unrepresentative parliamentary system established by the constitution of December 1867, with the agreement of Austrian liberals. In separate essays Peter Hány and György Ránki (Budapest) utilize quantitative data and the concept of economic backwardness to maintain that the Compromise of 1867 was a variant of the bourgeois-capitalist transformation of state and society, which, though not the best, nevertheless was conducive to economic growth. Both historians reject the idea of a direct connection between economic growth and social and national development, a position that brought them into sharp conflict with the Romanian historian, Miron Constantinescu. George Macesich (Georgia) attempts, with the help of modern market theory (but with no empirical economic data), to confirm Oscar Jaszi's judgment that the compromise prevented the growth of an integrated economic unit. Though Macesich is less persuasive than Hány and Ránki, all three demonstrate how much can be learned from the application of modern economic analysis.

The third group of papers treats the response of the non-German and non-Magyar nationalities to the compromise and includes papers on small nationalities, such as Slovenes

and Slovaks, whose efforts to protect their national identities have been ignored until very recently. Erwin Melichar (Vienna) and Július Mésároš (Bratislava) get down to the nitty-gritty of the nationalities question. Melichar, a specialist in constitutional and administrative law, uses the decisions of the Austrian *Reichsgericht* from 1869 to 1918 to examine the implementation of Article 19 of the Austrian Constitution of 1867, which guaranteed nationality rights. The attempts of the court to specify what constituted a denial of nationality rights reveal the strengths and weaknesses of Article 19 and of the *Reichsgericht* itself. In his article, which is studded with demographic tables, Mésároš analyzes the nationalities question at its most sensitive point—the growth and diffusion of educational institutions. In an innovative comparative article, Peter Sugar (Washington) compares and contrasts the reactions of the Croats, Romanians, and Slovaks in the period from 1867 to 1875. He shows that the Croats, with an "independent political history," were far better off in the cultural manifestations of their lives than the Romanians and the Slovaks, neither of whom had such a history. Paradoxically, however, the Croats, despite having "a political history," were not able to wrest much more of a substantive political nature from the Magyars than were the Slovaks and Romanians.

The papers in the fourth section concern the problem of federalism in Austria-Hungary. Hans Lentze (Vienna) concludes that a real federalization of Austria would not have been possible without depriving the Crown, the ruling classes, and the German-directed bureaucracy of their power, and that this would not have been possible without a struggle. Ferenc Pecze (Budapest) develops the thesis that the generally held view that Czech nationalistic aspirations in the 1870s were frustrated by the Magyars is an oversimplification. In a paper that sparked a lively debate, Joachim Remak (Santa Barbara) argues that the monarchy in 1914 was a viable state that might have survived and transformed itself from within had it not been "killed" by bad diplomacy and the war. Set against the papers of, among others, Lentze, Pecze, Mommsen, Goldinger, and Hány, as well as the critical remarks of several discussants, Remak's argument is unconvincing.

If there is disagreement among the participants on particulars, there is some agreement on larger issues. Until the late nineteenth century the Habsburg monarchy provided a framework for economic growth, expansion of the social structure, and cultural growth that benefited all of East Central Europe. After that time, under conditions created by forces that the monarchy helped to generate and organize, in part by the Compromise of 1867, the traditional sociopolitical foundations of the Habsburg monarchy and the compromise itself became obstacles to a more equitable diffusion of economic growth as well as to fully differentiated modern social structures. Most of the participants agree that the dissolution of the monarchy was inevitable, not according to any abstract notion of historical inevitability, but only within the context of comparative and systematic studies of European social-historical development. In this context the question of the inevitability of the monarchy's dissolution is shorn of its recriminatory overtones.

We are indebted to the editors and to the Slovakian Academy of Sciences for making the papers of the conference available to a larger readership, by which I mean not only historians of the Habsburg monarchy and East Central Europe, but all historians and social scientists interested in the process of "national awakening" among small nationalities, the transition from economically underdeveloped to economically developed societies, and the problem of coordinating national societies with international or transnational organizations. All of these are now global problems, for the investigation of which these papers provide comparative instances. The value of the volume is enhanced by a list of newspapers and periodicals mentioned in the papers and discussions and by an index of names of historical personages and authors. In view of its merits, the large number of typographical errors and typographically garbled sentences may be regarded as simply annoying. The editors and the printers faced a formidable task in producing a book of essays in four languages: German (21), English (10), French (8), and Russian (3).

Robert Musil observed that "the mysteries of dualism are at least as difficult to understand as those of the Trinity." The essays in this volume leave the mysteries of the Trinity un-

touched, but they do illumine those of the Compromise of 1867.

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YVES COLLART. *Le parti socialiste suisse et l'Internationale, 1914-1915: De l'Union nationale à Zimmerwald*. (Publications de l'Institut Universitaire de Hautes Études Internationales, Number 49.) Geneva: [the Institut;] distrib. by Librairie Droz, Geneva. 1969. Pp. xi, 373. 60 fr. S.

Collart's study asks why the truce of 1914 was broken only a year later, especially by people who had supported it with full conviction in 1914. He carefully analyzes all previous research theses, checks them on the basis of any and all available sources, rejects previous theses, and carefully justifies his reasons for new interpretations. The most important point is the meaning that Collart attributes to the Conference of Lugano, which was neglected in previous research. In this conference he perceives the indispensable connection for the transition from the truce to Zimmerwald. Collart's inquiry proceeds in three stages: an analysis of the truce, an assessment of the motives and the consequences of the Conference of Lugano, and the question why the change of climate took place in 1915, manifesting itself in the Zimmerwald Conference. The author attempts to show to what extent such decisions are determined by historical constants (constants of national and international development) and where and why certain variables are decisive for the choice of a specific course.

According to Collart the *salto mortale* of the Swiss truce of 1914 differs from that executed by the Social Democrats of the belligerent countries. It was justified more rationally than emotionally. It was dictated by the concern for the food supplies of a country largely cut off from international supply lines. With respect to Lugano Collart considers the duty of a small, neutral state, spared by the war, to work for peace and restitution of the International to have been the main motive for the turnabout that led from the truce to Zimmerwald. Collart is correct in concentrating not merely on the fact of this change but in inquiring after the tortuous ways and the various tempi by which it took place. The author is undoubt-

edly justified in selecting Robert Grimm as the key figure involved in this change of climate and in attempting to throw light on his career, hitherto not at all well known. Collart nails Grimm on the formula that the struggle for international peace is identical with forcing the national class struggle. I rather doubt that the change can be explained in so monocausal a fashion. Insofar as the truce was motivated by the material safeguards of the Swiss workers, it must have lost its *raison d'être* to the same degree that the working masses were pauperized and proletarianized by the unsolved distribution problem.

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BRIAN PULLAN. *Rich and Poor in Renaissance Venice: The Social Institutions of a Catholic State, to 1620*. Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 689. \$13.50.

Drawing upon a staggering amount of archival material Pullan organizes his book around three institutions that dealt with poor relief in Renaissance Venice—lay religious societies, hospitals, and moneylending. First he traces the development of the Scuole Grandi from their thirteenth-century beginnings as flagellant societies to their prestigious status as quasi-public charitable institutions. By the fourteenth century each Scuola consisted of two orders, the poor brethren who received charity and performed the devotional acts of the society, including ceremonial scourging, and the rich nonflagellating brothers who dispensed charity and took part in the pageantry of public festivals. Typically Venetian, the Scuole offered something for almost everyone—religious merit and social prestige for the rich non-nobles who ran them, charity for the respectable poor who did the work, and, as always, support for the state, for the Scuole loaned money to the treasury and provided rowers for the galleys. Only the truly unfortunate were overlooked—prostitutes, beggars, vagrants, and needy foreigners who were, literally, outcasts, for they had no access to the all-important charitable societies. Hospital care, the subject of Pullan's second section, was likewise unavailable to them, since the responsibility for institutional care was left to private societies, which natu-

rally favored their own members. By the late fifteenth century, however, the swelling numbers of war refugees, syphilitics, and sufferers of plague and famine were forcing the attention of state officials and high churchmen. Pressed by her enemies Venice was anxious to earn divine protection by charitable acts, while Franciscan preachers and Christian humanists alike were bringing the claims of the poor and the unfortunate to public consideration. Besides, there was a perceived threat of public disorder if the poor were not regulated and provided for. On the whole the effort was to direct the poor into socially useful occupations. This led to a distinction between the deserving poor and the incorrigibly idle. In some ways the new poor laws of the sixteenth century replaced the older benign neglect with harsh treatment—Tawney's "new medicine for poverty" was not exclusively a Puritan discovery.

In the third section Pullan studies the Venetian experience with that typically late medieval remedy for poverty, the provision of cheap credit. While most of her neighbors were establishing public loan funds, the famous *Monti di Pietà*, Venice continued to favor another common medieval expedient, the licensing of Jewish moneylenders. Jews could also be tithed, taxed, and intermittently shaken down for contributions to the fisc. This inevitably caused them to pass the cost of doing business on to the consumer in the form of high interest rates; but after 1573, apparently to celebrate the victory of Lepanto, the Venetians hit upon the expedient of forcing the Jews to maintain a non-profit loan bank for needy borrowers, a kind of Jewish *Monte di Pietà*. This was not a sign of greater Venetian sympathy for the poor but rather that the state's relentless squeezing of the Jews had so dried up Jewish capital that it was no longer a significant factor to the treasury, so what was left could be diverted to loans for the poor.

Pullan's book suffers from lack of focus and excessive detail but it contributes much new material not only for the subject of poor relief but also for the history of Venetian piety, social and political organization, and the treatment of the Jews. With respect to its place in the current lively discussion of the origins of early modern philanthropy, it comes down on the side of continuity. Pullan shows that in

Venice many sixteenth-century "innovations" were really efforts to revive earlier practices, that the tendency to question the spiritual value of voluntary poverty and to replace it with the new social and moral value of work was not limited to Protestant societies nor was it entirely new in the sixteenth century, and that social demands as well as evangelical doctrine could explain the formulation of a new work ethic. At the same time Pullan sees no thoroughly secular, social approach to poverty emerging among the Venetians, who continued to regard charity as a spiritual act that earned divine favor both for individuals and the state. Preservation of the existing order was ever the foremost consideration, into which the new spiritual and social values were absorbed. Was Venice unique in this? Pullan is willing to allow that it may have been, but one school of thought suggests that the subordination of welfare policy to the interests of the ruling class and "the national interest" has been and continues to be the dominant pattern in Western society.

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VITTOR IVO COMPARATO. *Giuseppe Valletta: Un intellettuale napoletano della fine del Seicento*. Naples: Istituto Italiano per gli Studi Storici. 1970. Pp. 360. L. 5,000.

The seventeenth century in Italy between the condemnation of Galileo and the arrival of Vico has been a *mare sconosciuto* until recent studies of Badaloni, Mastellone, and others have shown that a good deal was going on, especially in Naples. Here a group of lawyers, including Giuseppe Valletta (1636–1714), keenly debated the issues of the day. The son of a poor tailor, Valletta became a wealthy lawyer and merchant with a keen mind and great erudition, which he applied in a number of treatises.

Comparato successfully argues that at the center of Valletta's thought was a conception of natural law derived primarily from Grotius and the late sixteenth-century French jurists, above all Bodin. In all of his thought Valletta was a man of European culture who transcended purely Italian concerns. In his politi-

cal thought Valletta moved beyond traditional Italian theory, still focused on how to acquire and conserve states, to the European base of seeking the balance between the sovereign's authority and the subject's rights. Valletta argued against a monetary devaluation proposed by the Spanish government on the principle that the economy was founded on natural laws of exchange and should be allowed to function freely. In a critique of the Inquisition Valletta argued that it violated procedurally the natural right of defense and betrayed historically its roots. Sarpi had argued that the state rather than the Church should conduct it; Valletta took the critique a step further by balancing the principle of free inquiry against the duty of the public authority to maintain unity. This led logically to Valletta's defense of the right of philosophical inquiry, with its distinction between theological truth based on authority and philosophical truth founded on investigation of nature. In summary, Valletta appears as a transitional figure, anchored in Renaissance legal humanism and historical erudition while only a step away from the Enlightenment.

Comparato has combed the sources for Valletta's printed and manuscript sources, printing two of the latter in appendixes. In each chapter Comparato sets the stage by copious reference to other contemporary treatises, and he finishes by explaining the sources that went into Valletta's analysis. This is a useful and competent study in an area increasingly valued as important in the history of European thought.

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DENIS MACK SMITH. *Victor Emanuel, Cavour, and the Risorgimento*. New York: Oxford University Press. 1971. Pp. xv, 381. \$22.50.

The history of the Risorgimento has never been the same since Denis Mack Smith began revisiting it about twenty years ago. The Risorgimento was still in the midst of the neo-Marxist revisionist storm raised by the publication of Antonio Gramsci's *Quaderni del Carcere* (1947) when Mack Smith's disturbing monographic study in political conflict entitled *Cavour and Garibaldi, 1860* (1954) and then, four years later, his general history of modern Italy

brought a fresh whirlwind of historiographical controversy. Though it would be far from irrelevant to write a full critical review of Mack Smith's present book on Victor Emmanuel and Cavour, unfortunately not even a brief summary of the highlights of that controversy can be attempted here.

Following his fruitful detour into Sicilian regional history, which I feel relatively certain was both cause and effect of his immersion in Garibaldi's liberation movement of the island in 1860, Mack Smith has now returned to the high road of Italian national history running from the dead end of the revolutions of 1848-49 to the Piedmontese march on Rome in 1870. On the basis of part of the older documentation enlivened by research in hitherto unexplored or unavailable source materials from Italian, British, French, and Austrian archival collections, Mack Smith presents his findings and views, old and new, concerning the two culminating decades of the Risorgimento. This time the Italian national revolution is analyzed from a variety of aspects that, on the whole, are subsumed under "the personal contribution which two very different individuals [Victor Emmanuel and Cavour]—the two most politically powerful men of the Risorgimento—made to the way that Italy became a nation" (p. xi). Had Mack Smith been fortunate, as he had long hoped, in having access to the archives of the former Italian royal house that now lie buried at Cascais in Portugal where they were mysteriously transferred after Italy became a republic in 1946, he would undoubtedly have written a much-needed critical biography of Victor Emmanuel II. Whether or not one agrees with one of Mack Smith's major historical assumptions (and, for a variety of reasons, I do only in part), for him Victor Emmanuel ranks higher in influence as well as in position than Garibaldi and Mazzini and merely on a par with, not below, Cavour as a maker of united Italy. The impact of Victor Emmanuel's personal and dynastic legacy, almost wholly negative, persisted beyond the fall of fascism. The first king of Italy is thus studied by Mack Smith as a primal molder of policies, practices, and institutions (chiefly a powerful and "irresponsible" executive) that left an inescapable impress on subsequent national developments and that were almost deterministi-

cally crystallized into a royal "residual power," which allowed Victor Emmanuel's son and grandson to intervene decisively at crucial moments in later Italian history in support of such "strong men" as Crispi, Salandra, Musso- lini, and Marshal Badoglio.

After an introductory overview of Risorgimento history from 1840 to 1870 and through fourteen self-contained but more or less interrelated essays, Mack Smith engages in a sort of clinical dissection of various aspects of his fundamental thesis on the centrality of Victor Emmanuel's negative "contribution" to the making, structuring, and activities of the Italian unitary state. In a series of masterly chapters Mack Smith re-envelops the diplomacy of the Risorgimento within the contradictions of its multiple, or at least dualistic, political leadership—Villafranca in July 1859 and Marsala in May 1860 proved only the most dramatic revelations of the nemesis that hounded the unresolved conflict between Victor Emmanuel and Cavour—and reveals that the sources of the erosive disaccord lay infinitely less in the much publicized clash of strong personalities than in recurring confrontations between the constitutional prerogatives of the king and the institutional and parliamentary responsibilities of the prime minister. For, in essence, they were engaged in the Italian version of the ancient struggle between monarchy and representative government, old dynastic ambition and emerging national interest. Thus Mack Smith subtly succeeds in stripping down some of the most resistant hagiographic veils with which a prominent sector of "traditional" Italian historiography has tended to cover up, ignore, minimize, or obviate with almost equal generosity the personal flaws and the grave, sometimes tragic, official errors of the *Re Galantuomo*.

In this volume practically none of the major historical viewpoints on the Risorgimento that have come to be associated with Mack Smith is either substantively or radically changed. Nevertheless, the total effect of this fascinating prismatic study of the politics of the Risorgimento has proved to be, at least for one reader, much more positive, paradoxically even in its negative emphases, than its author's general history of modern Italy. Whatever his larger intellectual and cultural interests, there can hardly be any question that Mack Smith is at

his unequalled best with monographic political history, even if, as may perhaps be unavoidable for all professionals, he sometimes appears to become too enchanted and, therefore, entrapped by the very sources he can so deftly exploit. Yet even this judgment needs drastic modification, at least in the light of his superb two-volume history of modern Sicily, a miniature model of which is reproduced in the present volume with the publication, I believe for the first time in English, of his 1950 article in Italian on the peasants' revolt in Sicily in 1860. Interestingly enough, it is in this essay, which at first sight does not seem to be directly connected with the main themes of a volume on Victor Emmanuel and Cavour, that Mack Smith reiterates the core of his views on the Risorgimento. For him, the Italian national revolution is still essentially a multiple civil war, which, appropriated as it was by the forces of law and order represented by the Savoyard monarchy and the liberal middle classes, excluded the masses of the Italian people, urban and peasant alike, from "the fruits of their own success" in bringing down the institutional, social, and moral pillars of the various *anciens régimes* in Italy.

Mack Smith quite rightly believes that "there is no necessary reason why truth should be beautiful or simple" (p. 176). The studies in this volume make it amply evident that the "truth" about Victor Emmanuel II is almost absolutely harsher than some "traditional" Italian historians ever acknowledged—was the king perhaps "half mad," as Sir James Hudson reported from Turin to the Foreign Office at a moment of exasperation and frustration in 1862? On the other hand, when the truth about Cavour's diplomatic, political, and constitutional thought and action is pitted, as it essentially is in this volume, against the king's anachronistic and often dangerous and irresponsible activities, it shows something finer, richer, more human and historically fertile than that almost demonic face of the Machiavellian "fox" that the relatively isolated confrontation between Cavour and Garibaldi in 1860 had tended to reveal. In this volume Mack Smith has neither simplified nor beautified historical truth as he sees it. Nevertheless, by placing the "truth" within a larger context of historiographical perspectives he has tended to render

his reinterpretation of the Risorgimento no less original and brilliant, but certainly mellowed and perhaps wiser than his almost single-mindedly iconoclastic earlier work in modern Italian history.

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GIORGIO SPINI. *L'Evangelo e il berretto frigio: Storia della Chiesa Cristiana Libera in Italia, 1870-1904*. (Storia del movimento evangelico in Italia, Number 1.) Turin: Editrice Claudiana. [1971.] Pp. 265. Cloth L. 3,500, paper L. 2,800.

This volume dwells upon an often forgotten aspect of the Risorgimento—the Protestant awakening that accompanied the political reorganization. Specifically, it examines the development of the Free Christian Church in Italy from its appearance in 1870 to its dissolution in 1904. Identified with the Left and committed to the independence of each congregation, its ministers, as Spini shows, found themselves not only at odds with the Catholics but with the Waldenses as well, and hence determined to march their separate way.

The Free Church, according to Spini, received its inspiration from two sources: the Anglo-Saxon religious revival and the Risorgimento. Its fatal flaw was the inability to reconcile the two. Superficially the problem seemed resolved, for the foreign sponsors of the institution joined nationals in pressing for a close association with the state. The church achieved its respectability by sacrificing its vitality, for its preoccupation to please its foreign paymasters and the political establishment repelled its most devout members. Thus, though the church could boast of some two thousand communicants in the 1880s, a doubling of its membership since unification, it was no longer free or Italian.

The specialist will appreciate the detailed descriptions of the thirty communities in the church, the comprehensive coverage of the activities of their pastors, the reports of their general assemblies, and the analysis of their abortive attempt to merge with the Waldensian Church. These, when combined with the writings of a number of church members, create the picture of a dedicated group of men working against clerical pressure and widespread ignorance to effect a religious transformation.

Unfortunately Spini relies almost exclusively upon evangelical sources, and thus has produced a restricted and one-sided study of the Free Church, which after 1890 came to be called the Italian Evangelical Church. We are given but a glimpse of the manner in which the political elite, the various classes, and the vast majority of Italians reacted to that institution. Little is said of the means by which this church proposed to draw the masses away from the traditional faith and into its own ranks. What is presented, instead, is a survey of the maneuvers of its leaders and a catalog of the activities of its various congregations.

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Atti del XLIV Congresso di storia del Risorgimento italiano (Trieste, 31 ottobre-4 novembre 1968) (La fine della prima guerra mondiale e i problemi relativi). (Istituto per la Storia del Risorgimento Italiano. Biblioteca scientifica. Atti dei Congressi, Volume 12.) Rome: the Istituto. 1970. Pp. 269.

How much valuable work of able scholars is lost when it appears in volumes with bland serial titles such as this? Historians of Italy are doubtless attentive to publications of the Istituto per la Storia del Risorgimento, but the proceedings of its Forty-fourth Congress have a much broader appeal and suggest once more the crisis of information retrieval in this age of proliferating knowledge.

These seven papers were presented in Trieste in the autumn of 1968 when the Istituto commemorated the sesquicentennial of that Adriatic city's unification with Italy. The theme of the meetings, "La fine della prima guerra mondiale e i problemi relativi," was quite appropriate and opportune, but it becomes almost antiseptic as the subtitle of such a rich collection of information and observations. Hence one initially has the impression that the congress undertook a panoramic consideration of European events in 1918 and gave the expected emphasis to Italian affairs at the close of the war. But closer examination shows that the participants produced an excellent résumé of numerous aspects of World War I that was based on recent research. The résumé

has also taken into account the significance of the forgotten, maligned, or misunderstood Italian front.

As in any such collection, the essays vary in quality, but unlike most such assemblages, each of these works has something of unusual value. Aldo Garosci, in his "Mutazioni di equilibri e ideali politici nel corso della prima guerra mondiale," has presented a truly stimulating, even seminal work, which treats anew the causes of the conflict and the alteration or adaptation of each belligerent's war aims. He has drawn most imaginatively upon Fritz Fischer and Arno Mayer, as well as Croce, and goes beyond all. Yet potentially more controversial are the assertions by the French historian Henry Contamine, who asserts that the events of the Italian front shortened the war and perhaps held the key to its outcome. His evidence and arguments will not be easily refuted.

Some vital and easily neglected aspects of the dissolution of the Austro-Hungarian Empire (for example, issues deriving from transmission of administrative responsibilities) are treated by Fritz Fellner, whose remarks spurred one listener to warn against excessive criticism of Austrian relations with the successor states of the Empire.

Luigi De Rosa has provided a sweeping view of the tragic and even cataclysmic economic consequences of the Great War. If he may be faulted for giving less attention to Russia, England, and France, De Rosa has nevertheless provided unusual information on such countries as Finland, Spain, Bulgaria, and Sweden. His conclusion on the Continental situation is most candid: Europe should have given more attention to helping itself by creating its own markets after the war. Manlio Udina, a jurist, has undertaken a similar but less penetrating analysis of the drastically altered and wrenched situation of international law and organizations in the postwar world.

Leo Valiani's lively essay on the stormy course of Italian domestic politics and foreign policy in 1918 demonstrates the fragility and uncertainty of the country's commitment to the Treaty of London long before the controversy over that pact erupted and threatened Allied solidarity at Versailles. This account ought well to be considered by any future historians of the peace conference, especially those who

would reappraise America's policies toward Italy in 1919.

The late Carlo Schiffrer, in "L'attesa di Trieste," evokes the enthusiasm of Trieste on the eve of its capture by the Italian army. Still, he has not overlooked the history of the city with its Slavic minority, its vital links to its non-Italian economic hinterland, and its romantic ties to the Risorgimento, questions that have continued to make news long since 1918.

Not long ago Jean-Baptiste Duroselle remarked that a new generation of works on World War I, with a different perspective, could soon be expected. Here is some evidence of his accuracy; and it is therefore to be hoped that at least one, if not more, of these essays may become the basis for new monographs.

BENJAMIN F. BROWN
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STANISŁAW GRODZISKI. *Historia ustroju społeczno politycznego Galicji, 1772-1848* [A History of the Social and Political Organization of Galicia, 1772-1848]. (Polska Akademia Nauk—Oddział w Krakowie. Prace Komisji Nauk Historycznych, Number 28.) Cracow: Zakład Narodowy im. Ossolińskich, Wydawnictwo Polskiej Akademii Nauk. 1971. Pp. 303. Zł. 60.

This book on Galicia pertains to Poland and to Austria, which acquired Galicia in the first and third partitions of Poland. It provides a thorough study of the historical processes in that province with a pluralistic society and diverse nationalities, religions, and economic conditions. The author, being a historian of law (a group of scholars particularly prominent in Europe) concentrates on the legal aspect of the subject.

A detailed survey of sources of law and a geographical description of the territory open this study. Then the author analyzes social relations, including the nobility; cities and burghers and their professional and occupational problems; the clergy as a social group; and peasants and the attempted reforms at their emancipation, their self-government, and their rebellion of 1846. Colonization and Germanization of Galicia and the place and fate of Jews complement this part of the book. The next part brings forth a description of religious relations followed by an analysis of political and administrative organization of Galicia in

the framework of the Austrian monarchy and empire. The organization and functioning of the judicial system is a new major part of the book. The fiscal order and problems, taxation, and other economic matters are treated in the next chapter. Separate consideration is given to the organization of armed forces. One chapter is entirely devoted to an analysis of education and political thought in Galicia. The study is ended with concise conclusions and is supplied with a summary in German. All problems examined in this monograph are treated with respect to their historical development in the period from 1772 until 1848.

The study is firmly set in a broad source basis. The author fully uses archival materials as well as broad secondary sources. A fresh insight is often provided by the memoirs of contemporaries. This solid scholarly apparatus is reflected in numerous pertinent references.

This is the most up-to-date study of the problem. Together with K. Grzybowski's *Galicja 1848-1914* (1959), this book completes our knowledge of that part of Poland in the period of partitions until World War I. It also contributes to a better knowledge of the Austrian Empire as well as to a deeper understanding of European history in the period of expansion of some states and in the areas of struggle of conquered nations for autonomy and their own organized way of life.

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LOUIS FITZGIBBON. *Katyn*. Introduction by CONSTANTINE FITZGIBBON. New York: Charles Scribner's Sons. 1971. Pp. 285. \$10.00.

During the invasion of Poland in 1939 the Russians took prisoner about fifteen thousand Polish officers and placed them in the prisoner of war camps at Kozielsk, Starobielsk, and Ostashkov. After the spring of 1940 all but a few hundred of these officers disappeared and nothing was heard of them. More than a year later, after the German invasion of the Soviet Union, the Polish exile government in London concluded an agreement with Moscow that provided for an "amnesty" for the Poles in the Soviet Union and for the formation of a Polish army there. When most of the Polish officers

who had fallen into Soviet hands in 1939 failed to report to the Polish army, the Poles, naturally, inquired about them only to be told by the Soviet authorities that they knew nothing of their whereabouts. Then, on April 13, 1943, the Germans announced that they found the bodies of some forty-three hundred Polish officers buried in Katyn wood near Smolensk. Each man had been shot in the back of the head. The Germans at once accused their Soviet enemy of perpetrating this monstrous crime. The Russians immediately counter-charged that the Germans had done it. When the Polish government asked the International Red Cross to investigate the Katyn massacre, Moscow broke diplomatic relations with that government.

Louis Fitzgibbon states that he has written the book to "clear once and for all the obscurity which has shrouded for over thirty years the worst crime against prisoners-of-war ever committed, and perhaps the worst single unpunished crime in history." He has, to be sure, uncovered no new evidence, and it is doubtful that his book will have a greater impact than the books on Katyn published previously. There has been little doubt from the beginning that the Soviets perpetrated the Katyn crime. Nonetheless, this horrible deed needs to be exposed again and again. The book consists mostly of extensive quotations from the testimonies of the former Polish prisoners of war and other documents. It also contains twenty pages of most gruesome photographs and the list of the 4,143 victims identified at Katyn. This is a thorough and well-written book.

ZYGMUNT J. GASIOROWSKI
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FERENC A. VÁLI. *Bridge across the Bosphorus: The Foreign Policy of Turkey*. Baltimore: Johns Hopkins Press. 1971. Pp. xiv, 410. \$12.50.

Váli's thesis is that modern Turkey pursues three fundamental goals: national security, economic development, and "Europeanization." Policies adopted in their pursuit have changed, but the goals (of which Váli approves) remain unaltered since Atatürk.

This analysis permits Váli to applaud much of current Turkish foreign policy: cordial but

restrained relations with Russia, close but dignified relations with the United States, NATO membership, Common Market affiliation, limited pro-Arab support against Israel, and advocacy of peace in the Middle East. Váli supports the "magnificent scheme" to bridge the Bosphorus, to him symbolic of Turkey's third goal, to be assimilated into European civilization. Váli's research in these areas seems competent, informative, and well documented, if marred by repetition.

Váli is weaker in his attempt to provide "the proper historical setting"—fourteen pages for six hundred years of Ottoman foreign policy. Such brief coverage results from his belief that the Ottoman legacy is minimal. Ottoman policies were "hesitating, haphazard, and even irrational," dependent on "concessions, 'appeasement,' or humiliating surrender," whereas those of modern Turkey supposedly represent a near-total discontinuity. Where Váli has provided additional Ottoman historical background the results are commendable, as are his discussions of Soviet-Turkish relations and of the Cyprus issue. Where he has not, as in his sketch of economic development, his conclusions go astray. Two examples: Ottoman leaders purportedly attempted to develop military technology while ignoring overall economic and industrial development. In actuality low import duties imposed upon the Porte in 1838 frustrated an extensive Ottoman effort to initiate its own industrial revolution in the 1840s. Economic development was thereafter impossible until Turkey regained control of tariff rates in 1929. Second, in attributing the origins of *étatisme* exclusively to Soviet models Váli ignores traditional Ottoman state workshops and heavy industry, including shipyards, foundries, smelters and mines, and several state factories that the Republic inherited. Similarly, by arguing that Turkey's current mixed economy is Western he again slights Ottoman precedents.

Two other disappointing features deserve comment. First, the index is an economy-model list of proper nouns capitalized in the text. One looks in vain for general references to agriculture, education, industry, political parties, trade, and so on. Second, the one map is insufficient—it includes no roads, railroads, population symbols, foreign military bases, or former political boundaries, and but one river.

Important regional and place names referred to in the text are omitted. These drawbacks, while annoying, are not critical. Váli has produced a valuable work.

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V. A. PETROV. *Ocherki po istorii revoliutsionnogo dvizheniia v russkoi armii v 1905 g.* [Essays on the History of the Revolutionary Movement in the Russian Army in 1905]. (Akademiia Nauk SSSR, Leningradskoe Otdelenie, Instituta Istorii.) Leningrad: Izdatel'stvo "Nauka." 1964. Pp. 426.

The modern period has never attracted the best Soviet historians, and in this arid field no subject has suffered more from lack of standards than the history of the army. Trained historians leave the subject to military men who regard themselves as historians. As a consequence, with the major exception of P. A. Zaionchkovskii's work on the military reforms of the 1860s and 1870s, there are no worthwhile Soviet studies on any aspect of the history of the Russian army.

Under the circumstances one opens every new book on the army with restrained hope. Petrov's book, however, disappoints even the most modest expectations. Let us take for granted the obligatory distortions (for example, the Mensheviks did nothing but undermine the revolutionary work of the Bolsheviks); even so, Petrov's inadequacy as a historian is distressing. He has no sense of what is important and what is trivial; he does not connect his little facts with larger issues; instead of constructing well-supported generalizations he paraphrases documents. Worst of all he fails to ask those questions that would make his collection of facts meaningful: did the revolutionary movement among the soldiers contribute to Russian defeat, what kind of soldiers were likely to participate in revolutionary action, how widespread was revolutionary sentiment, and why did the revolutionary movement ultimately collapse? The description of "revolutionary acts"—surely a vague concept—in regiment after regiment makes excruciatingly boring reading (for hundreds of pages).

There is, however, one fascinating chapter in the book that alone justifies the investment of

two rubles and five kopeks. In great detail Petrov paraphrases eighty-four petitions prepared by soldier groups. We have so few sources on the basis of which the world view of the Russian peasant (or in this case, soldier) could be reconstructed that this contribution is very valuable. The soldiers' demands were varied: some groups protested against being used as police in putting down disturbances, while others merely wanted to receive extra pay for this duty. Some regiments wanted libraries, while others asked for free soap or higher salaries for army musicians. Reading these petitions one inevitably gains sympathy for the Russian soldier, living in terrible misery, receiving forty-five kopeks every two months, constantly humiliated and abused by his officers. One catches a glimpse of the faces of those peasants and soldiers who twelve years later would bring down the Imperial regime.

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NEAR EAST

RICHARD G. HOVANNISIAN. *The Republic of Armenia. Volume 1, The First Year, 1918-1919.* (Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1971. Pp. xxiii, 547. \$15.00.

The present work is the sequel to Professor Hovannisian's previous study, *Armenia on the Road to Independence, 1918* (1967). Heavily documenting his book, the author presents in this the first of a projected three-volume analysis a detailed description of the trials and tribulations of the ill-fated Armenian Republic's first year (1918-19). Not only does he cover the republic's many domestic crises, but he also gives considerable treatment of its foreign relations. One can only sympathize with the enormous internal problems—lack of food and transport, disease and malnutrition, destruction and poverty on every hand—facing the republic's provisional government. The author also gives much attention to the republic's tortuous foreign relations, which only add to the complexities of its internal problems. He explores in considerable depth the republic's relations with Georgia and Azerbaidzhan, its Transcau-

casian neighbors, with the pro-Muslim British forces, with the White Russian troops of General Anton Denikin, and with the hostile nationalist Turkish forces of Mustafa Kemal. Attention is given the efforts of the republic's delegation to the Paris Peace Conference, where, in conjunction with the delegation representing the Turkish Armenians, it petitioned the powers to create an integrated Armenian state consisting of the Armenian Republic, located in Russian Armenia, and Turkish Armenia, located in the Ottoman Empire. The student of the Armenian mandate question and of the endeavors of American relief workers will also find ample space devoted to their particular interests.

No filiopietistic writing pervades this study. The Armenophile will, indeed, find the author's narrative both penetrating and objective as he tries to unfold the story of the republic's efforts to survive the first crucial year. In fact, one is at times inclined to feel the author is overly critical of the republic's officials. Presented in a chronological narrative, the author, in what will undoubtedly become the definitive study of the Armenian Republic, seeks to explore and evaluate the tragic turn of events that made independence for Armenia little more than a pipe dream.

Thoroughly researched on a multiarchival scale, this study is based on the author's search in primary sources in Russian, Turkish, German, English, and Armenian archival material. This book makes it apparent that the diplomatic historian must seek answers to his questions in more than one archival source in order to make a completely objective analysis.

While the author has accomplished his purpose of presenting "a detailed, if not involved, history of the Republic of Armenia," one is inclined to wonder whether a shorter treatment would not have sufficed to cover that ill-fated republic's first year of existence. In any event, this work will answer many questions for the scholar whose interest lies in Russian and Middle Eastern studies.

THOMAS A. BRYSON
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MEHDI HERAVI. *Iranian-American Diplomacy*. Brooklyn: Theo. Gaus' Sons. 1969. Pp. 161. \$6.50.

MICHAEL KAHL SHEEHAN. *Iran: The Impact of United States Interests and Policies 1941-1954*. Brooklyn: Theo. Gaus' Sons. 1968. Pp. xiv, 88. \$5.00.

Dr. Heravi's book, while providing an adequate general survey of United States relations with Iran, is both too brief and too lacking in guiding generalizations to be recommended with enthusiasm. It lacks an overall view of the changing nature of United States motives in its relations with Iran beyond speaking of the period before World War II as one of isolationism. The book opens with a discussion of missionary activity in Iran, which states misleadingly that "the most drastic change in the Iranian mind and attitude was brought about by the establishment of the first American missionary school in 1835. . . . The first formal school for girls, which was established by the American missionaries, also greatly improved the social standards of Iran."

The book lacks analytical statements of the bases of American policies over the long period it discusses. Many of the generalizations it includes are taken from the sources used by the author. A more comprehensive approach should have come out of studying the period. Minor errors occur, such as seeing Nasir ad-Din Shah as a frustrated reformer, or speaking in the plural of tobacco concessions to British corporations. Transliteration errors are also frequent, so that we get "Valihad" for (in simplified transliteration) Valiahd, and "Gas-Ri-Shirin" for Qasr-i Shirin. The author has, however, used a great deal of documentation to present the main facts about United States-Iranian relations; and except for the period better covered by Abraham Yeselson's *United States-Persian Diplomatic Relations 1883-1921* (1956), the book may be profitably referred to by students of the area as a useful general survey of the long period it covers.

Mr. Sheehan's book wisely concentrates on a far shorter period than Dr. Heravi's. Although based almost exclusively on published sources and secondary works and presenting little that is new to students of the area, the book may be recommended for its bringing together of materials that have not been systematically presented before. The major criticism to be made of it is that it generally takes U.S. policy at its own word and presents it as an increasing com-

mitment to Iran as a bastion of the "free world," which the United States has defended against Soviet aggression. It is correctly stated that the United States took over a position formerly occupied by Great Britain in Iran, but U.S. motivations are scarcely interpreted in terms other than those used by the American authorities themselves. The expansion of U.S. commitments abroad during and after World War II cannot be adequately understood by quoting the rhetoric of American self-justification. Both military and economic expansion were in the interest of establishing the dominant power and economic interests of the United States. Aside from these caveats Mr. Sheehan's book can be recommended as a very brief and readable overview of the development of the American interest from the period of open door policies, which had long characterized U.S. policy in Iran, to the year when American control became paramount. Mr. Sheehan does not hesitate to note that the Shah's coup in 1953 against Mossadeq was engineered by the CIA, even quoting Alan Dulles as implicitly admitting this. The book's documentation of the rapid growth of U.S. influence in Iran is cogent and welcome, and its generally accurate presentation of at least the external facts of United States relations with Iran make it an appropriate introduction to the questions it discusses.

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AFRICA

HUBERT DESCHAMPS, editor. *Histoire générale de l'Afrique noire, de Madagascar et des archipels*. Volume 2, *De 1800 à nos jours*. Paris: Presses Universitaires de France. 1971. Pp. 720.

With this volume Professor Deschamps and his colleagues complete the most significant French entry into the new generation of multi-authored, multivolumed histories of Africa. Though several of the projected entries (like the *Cambridge History of Africa* or the UNESCO history) are still to come, the task they face collectively and individually is that of assimilating the vast body of new research produced since the early 1950s, when Africa first

emerged as a "new" field of history. The effort itself raises a whole series of crucial questions—questions that should be asked, perhaps, of more traditional fields as well, but are raised here more acutely because these new histories of continental scope are appearing on a clean slate. Who, for example, are the intended readers? What background do they have? What criteria should govern the chronological and geographical distribution of space within the book? What kind of balance can be struck between the main lines of generalization and the detailed narrative of a local scene?

Deschamps's solution for this, his second volume, was to accept the conventional periodization for recent African history—1800–80 for the last phase of precolonial Africa, 1880–1945 for the high colonial period, 1945–70 for the decolonization and the early post colonial period. Within these three periods, he briefly outlines the broader aspects of African history as a whole, leaving individual regional specialists to deal each with his own region in his own way—and all too often without reference to what was going on elsewhere.

This relative lack of uniformity between chapters makes for even more unevenness than cooperative works usually show. Some authors see their task as an ordinary textbook presentation summarizing the current literature. Others take the opportunity to write original and sometimes brilliant synthetic accounts that go beyond anything now available in print. Yves Person's treatment of the region that centered on the upper Niger basin (essentially the Manding culture area) is a contribution of that kind. So, too, is Pierre Alexandre's treatment of the equatorial forest, which traces the broad sweep of nineteenth-century history for a region usually left out of textbooks. Claude Tardits and Pierre Kalk deal with the north central savanna from Cameroon and Lake Chad east to the Republic of the Sudan, another region badly neglected in anglophone histories of Africa. Jan Vansina's survey of the southern savanna is new in another way; it is used to update the version he presented in *Kingdoms of the Savanna* (1966). The history of Madagascar and the Mascarene Islands is not so clear an advance on the existing literature, but it is welcome simply because it is so often neglected.

Other regional chapters are far less adequate, and here the weakness of writing without a

common framework comes into its own. At worst synthesis is not even maintained at the regional level, and the regional accounts become a confusing jumble of unfamiliar names presented without explanation in the framework of a rigid narrative. The overabundance of unexplained data is especially striking in Pierre Mercier's treatment of the Guinea coast from Nigeria west to the Gold Coast, since Mercier is a sociologist; yet his account reads like an old-fashioned history text. Here and elsewhere authors who are not historians appear unfamiliar with the latest historical literature in English, as though the double barrier of language and discipline was too high a hurdle. The section on East Africa even has a remnant trace of the "Hamite" myth in the form of "caucasoid" cultural traits (pp. 265-66).

With the colonial period the organization changes. The twelve regional articles of the precolonial section are replaced by a new series centering on the colonial policy of the various European powers. This, unfortunately, shifts the emphasis to European aims and means, which must be dealt with before the authors even get around to the African reality. Most of them never reach that point, and their problem is heightened by Deschamps's allocation of space. More than half the volume is assigned to 1800-80, but only sixteen per cent to 1880-1945. In terms of pages per year, this distribution gives 4.6 for the period 1800-80, 7.4 for the period 1945-70, but only 1.7 for 1880-1945. This distribution seems to imply that the colonial period was not very important, but that assessment is never explicit, and it seems inexplicable for a book with a former colonial governor as editor and former colonial officials as at least a fifth of the other authors.

This idiosyncratic assignment of space continues in the allocations to the various colonial powers. Measured by the ratio of pages to present-day population, Liberia and the Spanish and Portuguese territories have twice the space of francophone Africa, while francophone territories in turn have double the space per capita assigned to anglophone Africa. I doubt that this was an intentional emphasis; it simply follows from the fact that it takes so much space to describe a colonial policy and administration, regardless of the number of people administered—and regardless of its actual influence on African his-

tory. The African response to these policies is seriously neglected.

The overemphasis on administrative and political history continues into the postcolonial period, joined now by occasional paragraphs on economic change. But social change, new tendencies in thought, religion, or art, and even economic change in the traditional sector are all underplayed. Instead, the emphasis of the section on 1945-70 falls very heavily on the 1960s and especially on the second half of that decade, so that history gives way to current events. More than half the discussion of Nigeria between 1945 and 1970 is devoted to the period 1966-70, explicable perhaps by the importance of the civil war, but more than half the space allotted to Ghana also deals with the period after 1960. Other spatial allocations for the colonial period are simply puzzling. It is possible that a francophone reader might want more space devoted to Madagascar than to South Africa—however difficult to justify in terms of the past or present place of either in African affairs. But why should the former British East Africa have more space than French West Africa and twice as much as British West Africa?

No reviewer has a right to ask for a book different from the one the author intended, but here is a general work, beautifully bound, printed, and illustrated. The physical setting suggests a work intended to be read now and used for reference over the years. Some of the precolonial sections have a lasting quality of the kind suggested, but the interpretation of the colonial period is just now being drastically revised by historians exploring the African side of the colonial experience. Five years from now the account given here will not be wrong, only beside the point. As for the postcolonial period, this version can hardly last beyond the time when the recent crises of the late 1960s have given way to still more recent crises of the early 1970s.

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ROBERT O. COLLINS, *Land beyond the Rivers: The Southern Sudan, 1898-1918*. New Haven: Yale University Press. 1971. Pp. xvi, 368. \$12.50.

This is the third volume of Professor Collins's

comprehensive history of the Southern Sudan, a subject that he has studied in some depth for the years 1883–1918. The present work deals with the years 1898–1918. Students of African and African-Arab history will welcome this contribution to an area that is nearly unmatched in its complexity and that remains little understood despite its importance in the dynamic of Middle-Eastern and African relations. Professor Collins's interest lies primarily in diplomatic and administrative history. He notes in his preface that early in his work he came to the conclusion that "the fundamental decisions affecting the conquest and administration of the Southern Sudan during the first decade of this century were largely the result of the Anglo-Congolese dispute over the Upper Nile." This interest in diplomatic history is reflected in the latest volume of this collection, although it is balanced by a just concern with the internal administrative policies of the British in the Southern Sudan. There is little sentimentality wasted over the motivation of British officers and consequently little effort to romanticize the laborious efforts to create a sensible and orderly administration. There is, on the other hand, a consistent attempt to understand the objectives of the administrators as well as the conditions they found, in a human and material sense. "British administrators in the Southern Sudan," writes Professor Collins, "regarded their role as fundamentally political. Modernization meant the more efficient operation of the administration rather than economic growth or raising the standard of living of the Southern Sudanese. The British were rulers first and developers second." Gentlemen remained gentlemen, and basing his observations on the class background of the British administrators, Professor Collins finds that "the paternalism of the squirearchy, not the incentives of the urban merchants, guided their actions."

I have selected one passage and limited my remarks, as is obvious, with the result that readers ought to feel deprived without reading more of Professor Collins for themselves. There is much here that deserves further comment and that will doubtless be variously interpreted. It seems highly regrettable to me that a work dealing with the Southern Sudan tells us so little of that internal history which the author was also interested in. Where are the Su-

danese? And what is this polite, discreet, imperialism that differs so markedly from the indifference and cruelty attributed to the others engaged in the same game—namely France and Belgium? Diplomatic and administrative histories are perhaps meant to whet the appetite particularly for those who are curious to know who the happy masses were that were being so efficiently administered.

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D. A. LOW. *Buganda in Modern History*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xii, 265. \$8.75.

D. A. LOW. *The Mind of Buganda: Documents of the Modern History of an African Kingdom*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xxvii, 234. \$8.50.

Professor Anthony Low has given us a volume of essays and an essential collection of documents centered on the political history of Buganda. These two volumes build upon his study of the making of the Uganda Agreement (*Buganda and British Overrule* [1960]) and the establishment of the Uganda Protectorate (in volume 2 of *Oxford History of East Africa* [1965]). In the scholarly tradition of Terence Ranger, Elizabeth Chilver, Yves Person, and Jacques Berque, Low demonstrates that detailed knowledge of the internal, precolonial history of an African society provides the most powerful and coherent explanation of colonial relationships and their aftermath. The essays, *Buganda in Modern History*, review and gather the political continuities in the history of Buganda between the 1880s and 1960s. They serve as a commentary upon *The Mind of Buganda*, a collection of sixty-two documents from the past century of Buganda history, put into context by a skillful fourteen-page introduction.

At this moment, when the Eurocentric view of the "discovery of the Nile" is again being given publicity on television, Professor Low's documents forcibly call to our attention the African view of imperial rivalry and colonial rule. The view from inside looking out reveals the complexities of interaction with intrusive British values and the cost of accommodation to political and cultural hegemony. Although

the Kabaka wrote in 1935 that "while boasting of having acquired Western education and civilization in an amazingly short period, we have entirely and completely ignored our native traditional customs," the documents also make clear that the Baganda achieved and wished to sustain a special political position in Uganda. They valued the symbolic, self-conscious unity provided by the "living and functioning form of the Kabakaship and Lukiiko" (the Lukiiko Memorandum, 1960), and they sensed great continuity in their four-hundred-year history.

There is a certain amount of perhaps unnecessary repetition among the seven essays, but the major insights cannot be faulted, and the total effect is cumulative. Just as in his selection of documents Professor Low sets out "the attitudes of mind which the Baganda have displayed on issues of major importance to them," so in his essays he keeps in focus the continuously complicated relationships between the Kingdom of Buganda and the larger protectorate, now the nation, of Uganda. The first essay shows clearly the political sophistication and adaptability of the Baganda in the final decades of the nineteenth century as they turn and use foreign religious elements for internal political purposes. The third essay argues that the British relied upon the "new men," the adaptive radicals, in the 1890s, but that in the changes following the Second World War the British relied upon the established leaders. Thus, at the very time when the hierarchy of senior chiefs was becoming more distant from the people, more bureaucratized, when "the personal nexus" between chiefs and people was atrophying, the British, too, failed to recognize the new generation. The fifth and sixth essays spell out the consequences: neotraditionalist dominance of the Lukiiko in 1957, the rise of new-style nationalist political parties, and the storming of the Kabaka's palace by Uganda government troops in 1966.

Perhaps the most self-contained essay is the fourth. It deals with the Hancock achievement at the Namirembe Conference of 1954. Through a study of the personalities involved and the successive positions taken, Professor Low reveals the thought and the logic used to open the possibility of a unitary constitution for Uganda. But because the Kabaka did not understand what was at stake and preferred to

think in the political metaphors of the past, "the carefully contrived opportunity was never seized, and a decade later nemesis followed."

PROSSER GIFFORD
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NORMAN ROBERT BENNETT. *Mirambo of Tanzania, 1840?-1884*. New York: Oxford University Press. 1971. Pp. xiii, 191. Cloth \$6.50, paper \$1.95.

A recent popular approach to the study of African history has been through biography. "Every nation needs to build its history around the exploits of great men," Mr. Bennett maintains. He has here refined and added to previously published work to present an account of the life of Mirambo, a Nyamwezi leader from western Tanzania who was a prominent military figure in the 1870s and 1880s. Mirambo has been principally known as a warrior and empire-builder who was a major political force in his own day but whose empire collapsed on his death; to this picture Mr. Bennett adds material on his diplomatic dealings and his relations with African, Arab, and European personalities. Mr. Bennett's command of the European source materials, principally those from British official and missionary archives, is admirable, and there seems little doubt that he has uncovered almost all available evidence. The problem is that it is European evidence; we see Mirambo almost invariably through European eyes and in terms of his contacts with Europeans. Very little African material is used. As a contribution to nineteenth-century Tanzanian history the work is useful but as biography it is inadequate, and Mirambo remains a shadow.

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ASIA AND THE EAST

CHANG KUO-T'AO. *The Rise of the Chinese Communist Party, 1921-1927*. (The Autobiography of Chang Kuo-t'ao, Volume 1.) Lawrence: University Press of Kansas. 1971. Pp. xxi, 756. \$25.00.

If China has a Trotsky, it is not Liu Shao-ch'i but Chang Kuo-t'ao. Chang broke with Mao Tse-tung and left the Chinese Communist

party (CCP) in 1938. His subsequent obscurity—and Mao's pre-eminence—has frustrated assessment of an outstanding revolutionist. The long-awaited publication of Chang's memoirs is an important event.

An organizer of the epoch-making student demonstration of May 4, 1919, Chang played a prominent role in the formation of the CCP. He pawned clothing to support the embryonic party organization and was a delegate to its first congress in 1921. After visiting the USSR and meeting Lenin, Chang narrowly escaped Wu P'ei-fu's brutal suppression of the Peking-Hankow railroad strike of 1923. On the evening of the historic incident of May 30, 1925, he arrived in Shanghai in time to organize the city's General Labor Union. By the conclusion of this volume he had shared in the hopes, disillusionments, fleeting victories, and bloody disasters that marked the first years of the CCP. At the age of thirty he was an established party leader.

Despite reservations, Chang welcomed Leninist organizational techniques but was shocked by the Comintern's heavy-handed intervention in the policies of the Chinese party. He recognized the fatal contradictions in Moscow's decision to integrate China's Communists into the Kuomintang. Though one may question his judgment that a bilateral united front might have led to "a normal multi-party democracy," his premonition of disaster was well founded. Yet how the Comintern developed its strangle hold over the CCP remains a mystery. Chang was not the only CCP leader to recognize the drawbacks of Moscow's stewardship. Why, then, did the party repeatedly capitulate to ill-conceived directives? In return for economic aid? (Chang says little on this.) Because the party was spellbound by the mystique of the Russian Revolution? (Chang and others who visited the USSR were disenchanted.) Was it the charisma of Soviet advisers? (Many of these men displayed abysmal ignorance of China, and even the formidable Borodin was no Rasputin.) Central to the problem is the prestigious Ch'en Tu-hsiu who became a scapegoat for Comintern failures after doggedly defending its distasteful policies. Yet after Ch'en's deposition a neophyte Comintern adviser was able to pick his successor. If anyone was in a position to understand why these things hap-

pened it was Chang Kuo-t'ao, yet his book leaves the mystery unsolved.

Chang's contributions heavily outweigh his omissions. Historians of the CCP have heretofore drawn upon party and Comintern documents. Painstaking dissection of doctrinal controversies has been highly abstract, and even Harold Isaacs's vivid *Tragedy of the Chinese Revolution* submerges individual protagonists in tides of mass movements. Chang has now written the most vital account of early CCP history since Isaacs's book appeared in 1938. This volume is, as A. Doak Barnett has observed, "a remarkable combination of involvement and dispassionate observation." Chang breathes life into fellow participants who have hitherto been little more than pasteboard mannequins—the imperious Comintern agent Maring, the party patriarch Ch'en Tu-hsiu, and the playboy revolutionist Ch'en Kung-po. Yet, in his guise of dispassionate observer, Chang fails to do justice to one key figure: himself. He moves through the battle-scarred landscape of the Chinese revolution noting others' foibles while his own record remains virtually unblemished. His thoughts and deeds are dutifully recorded but he remains a one-dimensional man. To bring Chang to life we will need the observations of others, including, of course, Mao Tse-tung. We eagerly await Chang Kuo-t'ao's second volume.

JOHN ISRAEL

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ARTHUR N. YOUNG. *China's Nation-Building Effort, 1927-1937: The Financial and Economic Record*. (Hoover Institution Publications 104.) [Stanford:] Hoover Institution Press, Stanford University. 1971. Pp. xx, 553. \$19.50.

This is Arthur Young's third large book on the economy of China during the period of Kuomintang (Nationalist party) rule. Where his earlier books dealt with foreign economic aid to China from 1937 to 1945 and with wartime finance and inflation, the present volume is a memoir and a history of the prewar efforts of the Nationalists to transform the system of public finance inherited in 1927 from the warlord Peking governments into an instrument for achieving national unity and (it was hoped) economic development. As financial adviser to the Republic of China from 1929 to 1947, the

author was more or less closely involved in the formulation of the financial policies of the Nanking and Chungking governments, so that he writes with some authority on the more technical aspects of fiscal and monetary affairs. From the records in his possession he occasionally offers valuable data not available elsewhere, for example, statements of the receipts and expenditures of the Nanking government for the fiscal years 1936 and 1937. Mr. Young, an honest reporter, acknowledges that his discussion of modernization and development (pp. 287-401) does "not have the depth of the treatment of fiscal and monetary matters with which I was concerned at first-hand and for which I have primary and often unique data."

The essential issue raised by this book is the classical one of the glass with some water in it: is what we have before us a vessel that is more accurately described as half empty or as half full? Mr. Young, in the summary passages that appear at several places in the text, agrees that the political and social development of China during the years 1927-37—which are not the main subject of his book—lagged behind the ability of the Nationalist government to rehabilitate and reorganize substantial parts of the modern sector of the economy. Thus central government revenues were increased from almost nil to one billion dollars Chinese currency, a modern budgetary and administrative system was organized, the market for internal borrowing was developed, most foreign debts in arrears were settled, the currency system was reformed and unified, a central bank was established, transport and communications were improved, and a modest program of development was begun with both domestic and foreign capital and technical aid. All were promising beginnings when compared to the period of warlordism that preceded. But, the author also notes, the rural areas were largely unchanged and their resources never tapped by the center, no substantial agrarian reform was undertaken, provincial and local governments were dominated by militarists, administration was frequently corrupt, the central government wasted its resources in civil wars, its political base was a narrowly circumscribed social elite, and the populace at large was inert and had no voice in the government.

After looking at the glass with some care Mr.

Young has decided that it was half full. Had war with Japan not come in 1937 the Kuomintang would not have been overthrown by its Communist party opponent. The prospect was for continued "evolutionary change and growth" on the basis of the relatively successful fiscal and monetary policies of the decade here considered. For many—indeed most—students of Kuomintang China, who are more skeptical about the efficacy of fiscal and monetary policy in coping with political and social crisis, the glass in question would be described as more than half empty in 1937 and leaking dangerously through numerous cracks.

ALBERT FEUERWERKER
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AMERICAS

GEORGE ATHAN BILLIAS and GERALD N. GROB, editors. *American History: Retrospect and Prospect*. New York: Free Press. 1971. Pp. viii, 471. Cloth \$7.95, paper \$4.95.

In the frequency and thoroughness of their self-scrutiny American historians must surely surpass all other practitioners of their craft. Theirs is the dedicated priesthood of a cult agreed upon only one dogma, "Know thyself." This proceeds in large part no doubt from phenomena which, to a European eye, appear distinctively American—the sheer volume of academic output and the number, diversity, and wide geographical scatter of the institutions that promote it. In such a situation the provision of formal channels of communication and their maintenance by regular dredging is a convenience almost amounting to a necessity. (Which should not, incidentally, diminish anyone's gratitude, least of all that of the foreign scholar, at finding this task so conscientiously and ably done.)

But behind the bibliographer's urge to order there is also the philosophic impulse to assess and evaluate. (No accident perhaps that "clerk," in its original and derived meanings, may cover both.) That the American historian sees himself—and is seen by many, perhaps most, of his readers—as the unacknowledged legislator of his times is almost as evident as that he is deemed to be the Rhadamanthus of

the national past. The pages of the *Newsletter* testify as vehemently to the first role as the pages of the *AHR* do to the second.

Moreover, the flexibility of the discipline is greater in the United States than elsewhere. It is more open to innovatory techniques and borrowed insights. There is therefore not only the social but, so to say, the technological, impact to be recorded. The American Clio is not only ready to sing songs of social significance; she is also prepared to try them on the twelve-tone scale or the Moog synthesizer. This may stimulate (it certainly does not impede) the rapid turnover of interpretations. Certainly the corpus of American history appears unique in its quick responsiveness to every shock wave that reaches it from the nation itself, so that today's national crisis becomes tomorrow's *nouvelle vague* of revisionism.

The authors of *American History* are in varying degrees responding to all these developments. They are seeking to update, in many fields, our swiftly obsolescing bibliographies. They are trying to record and relate the fresh evaluations of the American past that have been emerging in the sixties. And where relevant and practicable, they have been trying to do a historian's job on historiography, to enquire and explain where the new trends have come from, what they signify, where they may point. The result is a diversified, rich, and informative volume.

Two general impressions, irrespective of period or approach, remain from a reading of these very different essays. The first is the decline of great names. Certain figures, formative teachers, and interpreters do, of course, recur in these pages. But the age of the giants is over. There are no Turners or Beards. With the tragic losses of Hofstadter and Potter, even the ranks of the near-giants are sadly thinned. But a second impression is almost equally strong, of the great range and liveliness of the profession as a whole, of its impressive output, its restless energy, and its Oedipal addiction to patricide as a form of occupational therapy. Just as there are no giants, so there are no idols.

Less strong than an impression but more powerful than a whiff is a certain emanation from these pages of what an unkind critic would call insularity. The rich internal resources of the American past have encouraged

a certain exclusive concentration, even in our ecumenical present, on the American experience. There is rather too little evidence in these pages that the American historian's curiosity follows where his civic conscience or his vacation travel might lead him, to ask how far his findings about the American past might be enriched or clarified by being brought into comparison with the experience of other peoples at other times. We all wear blinders here; no one has panoramic vision. But because the American horizon is so broad it sometimes lends itself more readily to the illusion of being the totality of the perceptible.

The ten essays that make up *American History* fall into two categories—the chronological and the topical. All, that is, save Lee Benson's on "Middle Period Historiography: What Is To Be Done?" This, as its Lenin-esque interrogative might suggest, is as much an indictment as an inquiry. The research on 1816 to 1860 has, he charges, yielded only "unsystematic and trivial" information. But when one discovers the reason for this, namely that historians have obstinately declined to become social scientists, one realizes that the indictment is general. What we have here is a hard-hitting if rather old-fashioned restatement of the case for a scientific history, which could as reasonably be applied to any other period of the American past. Which is not to deny that Benson scores some very palpable hits, especially when he directs attention to our profession's sloppy use of terms and our inveterate addiction to "proof by haphazard quotation."

Next to Benson's, the widest-ranging essays are those by Samuel P. Hays on social history and James P. Baughman on economic and business history. Hays makes the large claim that the distinctiveness of social history lies not in its subject matter but in its way of looking at the past, concerning itself with "human interaction." What? Have we all been writing social history without knowing it? Not at least in its fullest realization, for that, as we are reminded apropos of immigration history, "requires that it be freed from the particular circumstances of time and place." If that seems beyond the reach of most of us poor prisoners of the here and now, we can nevertheless endorse much of what Hays has to say about getting social history away from its emphasis on

problem solving and its subservience to ideology, as also his warnings against the tendency to dichotomize.

Baughman's essay is a most illuminating survey of the ground recently cultivated by the economic historians with the new tools that they have derived from contemporary economic theory and, even more, with the new data that econometrics has put at their disposal. Here, it can hardly be disputed, there has been real progress. In 1944 the Committee on Research in Economic History of the Social Science Research Council identified three "target areas"—the roles of entrepreneurship and government in economic growth and the history of the firm. In all these, three solid advances have been made. It is not often that scholarship thus complies with its agenda.

Urban history, as a growth topic, receives appropriate scrutiny from Richard Wade, who writes not only a survey of past work and an agenda for future researchers but also a very perceptive and succinct history of American urbanization itself. For a declining trade, diplomatic history, Ernest May provides a comprehensive and far from tearful obituary, hopeful of the phoenix of international history that is rising from the ashes of the old documentation.

For many readers, however, the greatest practical utility may well be found in those essays that survey in a preponderantly bibliographical form recent work in separate periods. All these are well done, some in conspicuously comprehensive detail—like Jacob E. Cooke on the Federalist age—some with a greater freedom of choice and a more explicit commitment to one style of approach—like Gerald Grob, who hails the exhaustion of moralism in Reconstruction historiography and sees the future as belonging to a neo-David Donaldism. Thomas Barrow, surveying the colonial scene, is especially helpful in relating the significant periodical literature to the more familiar book-length studies. George Billias follows with a freshly categorized analysis of the large outpourings of the Revolutionary era, which constitute such an impressive run-up to 1976 and All That.

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HERMAN E. KROOSS and MARTIN R. BLYN. *A History of Financial Intermediaries*. (Random House Books in Finance.) New York: Random House, 1971. Pp. xi, 254. \$7.50.

In all fields of scholarly endeavor there has recently been a growing demand for the work of the synthesizer. Specialists of all types have found it almost impossible to keep abreast of the flow of articles and books without the help of summaries and syntheses. Market demand is fortunately being met in some areas of activity, and this history of financial intermediaries provides an example. In 246 pages the reader is offered a sweeping survey of the creation, the response to environmental changes (internal and external), innovative actions, variations in growth rates, and the general performance of financial intermediaries in the history of the United States. The book deals not only with commercial banks and mutual savings banks, as had earlier similar books, but also with insurance companies, general insurance companies, investment banks, credit unions, trust companies, savings and loan associations, noninsured pension funds, and investment companies. In comprehensiveness of coverage it is unique among histories of financial institutions. Those whose activities are analyzed now hold one-half of the total wealth of the nation, a fact that indicates their importance in the economy. As measures of growth of financial intermediaries, assets of banks and insurance companies approximated ten dollars per capita in 1800, and assets of the ten discussed now amount to about five thousand dollars per capita.

Although the reader must peruse carefully rather than breezily, the authors strive successfully to keep the narrative clear and straightforward. In the preface they explain why they concentrate on participants in this segment of economic life—on borrowers and lenders and the processes by which they were brought together. The introduction contains definitions of financial intermediary, the theoretical framework utilized, what financial intermediaries do, and their historical growth. Of particular concern is innovation in three areas—those encouraging saving, those easing the borrowing of funds, and those narrowing "the gap between the saver and the investor by improving liquidity and by adding to geographic mobility" (p. 4). In eight time periods from colonial times to

1970 the extent of and variations in innovations are analyzed—among others, the surges in innovative response occurring in 1816–36 and 1945–70. Twenty-one tables and three “figures” elucidate the text. The presence of a few typographical errors does not impair effectiveness of presentation, but both the index and the bibliography are so brief that they have limited utility. Minor limitations notwithstanding, this synthesis serves to remind the economic historian that assets of financial intermediaries have grown more rapidly than those of any other sector of the American economy. From it the noneconomic historian can learn where the financial power is and the process by which it got there.

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CONVERSE D. CLOWSE. *Economic Beginnings in Colonial South Carolina, 1670–1730*. (Tricentennial Studies, Number 3.) Columbia: University of South Carolina Press for the South Carolina Tricentennial Commission. 1971. Pp. ix, 283. \$6.95.

PAUL S. TAYLOR. *Georgia Plan: 1732–1752*. Berkeley: Institute of Business and Economic Research, Graduate School of Business Administration, University of California. 1972. Pp. xviii, 322. \$6.50.

During the third quarter of the eighteenth century the rice colonies of South Carolina and Georgia experienced an economic boom unparalleled among the British-American colonies since the emergence of the Caribbean sugar islands at the end of the seventeenth century. So great was the boom that on the eve of the Revolution the rice-producing regions of those colonies were almost certainly the most prosperous area on the British North American mainland and probably also the most promising area of agricultural investment in the whole of Britain's overseas dominions. Clearly written and thoroughly researched, these two volumes provide the fullest and most authoritative account ever published of the beginnings of this remarkable development in the socioeconomic foundations of South Carolina and Georgia.

For South Carolina those foundations were slowly laid. The central themes of its first two decades, Clowse makes clear, were the twin failures of the proprietors' elaborate schemes for

the development of the colony and of the search for a commercial crop. Trade in deer-skins and in surprisingly large numbers of Indian slaves with neighboring tribes generated some profits. But as late as 1690 the economic base of the colony was extremely precarious and the population correspondingly sparse. Only after 1690 did rice gradually emerge as a primary staple that provided, together with subsidized naval stores and the Indian trade, the profits necessary for the substantial expansion of the colony between 1705 and 1715. That decade was decisive in casting the socioeconomic life of the colony into a West Indian mold characterized by a small, slowly growing European population; a rapidly increasing number of African slaves, who accounted for a continuing upsurge in productivity as each slave produced approximately one ton of rice annually in addition to other subsidiary products; the concentration of good rice lands into the hands of a small white elite; and the emergence of the supporting mercantile groups necessary to perform the functions required by a plantation economy. Over the next fifteen years this expansion was slowed somewhat by a series of internal problems and adverse external conditions, but the heavy importation of African slaves continued, and rice received ever greater emphasis. Though economic troubles continued to hinder the colony for another decade, by 1730 it was poised on the threshold of the great boom that began in the 1740s as Carolinians gained access to larger international rice markets and developed indigo as a profitable second staple whose production was heavily subsidized by Britain.

There are several minor problems with the Clowse volume. Unfortunately it stops a full decade before the boom got underway, once again calling into question the wisdom of using political events, in this case the end of the proprietary, to determine the limits of social or economic studies. Moreover, Clowse's explanation for the introduction of slavery (the unsuitability of the climate for white labor) and his account of where Carolinians acquired the technical knowledge to produce rice (he does not consider the likely possibility that they learned it from their African slaves) are unsatisfactory, and the hard economic data on which the study is based are disappointingly

thin. Given the nature of the extant sources, however, it is doubtful that any future scholar will improve upon this study, and the author compensates for the lack of data with a series of responsible and informed projections.

Whereas the problem of the early South Carolinians was to find a viable commercial crop, the difficulty of the enterprising spirits among the first Georgians was to overcome the Trustees' opposition to their efforts to follow the Carolina example. In their attempt to create a colony of small, working farmers, the Trustees steadfastly clung to their original plan to exclude slavery, limit the size of landholdings, encourage white immigrants and the use of servant labor, and rely on public support for financing. The first twenty years of the colony's history, Taylor shows in an exhaustive and detailed narrative, were a perpetual tug of war between the Trustees and their supporters in the colony who were determined to keep Georgia from falling into the West Indian model and those other settlers who wanted desperately to emulate that model. A chronicle of the Trustees' gradual defeat on one point after another until their final relinquishment of the last important provisions of their plan in 1749, Taylor's volume is frankly revisionist in tone. He shows that the plan was more practical than earlier historians have suggested, that it worked quite well for over a decade, and that the Salzburgers, Scottish Highlanders, Dutch settlers, and other small farmers consistently supported the Trustees.

But the point is not that the plan was impractical but that it was out of harmony with the central animating spirit of the mid-eighteenth-century British Empire. For what both of these volumes illustrate so profusely is the extraordinary extent to which the momentum for colonial economic development was supplied not by proprietors or trustees in Britain but by the settlers on the spot and to which the primary impulse underlying that momentum "was not altruism or trial of a political system, but profits"—in Taylor's words, "the search for private gain." As many recent studies have implied, everywhere in the British-American colonies men's material appetites were continually whetted by the economic opportunities offered by an environment of abundance. Living next to the colony that at the very moment of the

founding of Georgia was on the verge of a dizzying economic boom and in an environment that was supremely suitable for following the Carolina example, Georgians would indeed have had to be Spartan to have resisted the temptation, in Egmont's disconsolate phrase, to become "rich in the ways of other provinces."

JACK P. GREENE

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SARAH MCCULLOH LEMMON, editor. *The Pettigrew Papers*. Volume 1, 1685–1818. Raleigh: State Department of Archives and History, 1971. Pp. xl, 699. \$15.00.

Volume 1 of the *Pettigrew Papers* contains the records of the early members of the Pettigrew family of the Albemarle Sound region of North Carolina. The principal items are the correspondence of the Reverend Charles Pettigrew, North Carolina's first elected (although not consecrated) bishop of the Episcopal Church and a successful colonial planter, and his only surviving son Ebenezer, an imaginative and prosperous planter himself.

After a brief but informative introduction by Dr. Sarah McCulloh Lemmon, biographer of Charles Pettigrew and the last of a series of editors who have worked on the papers, the records begin with wills dated 1685 and 1753; then follows the late eighteenth-century correspondence, which comprises the first section of the *Papers*. The records tell the story of an educated Philadelphian who came to eastern North Carolina and with devotion to the church and to his own business affairs rose quickly to a place of prominence in the Edenton area. The death of Charles Pettigrew's first wife, his remarriage, and the establishment of an estate on the southern part of the Albemarle Sound are recounted in a series of letters that includes a wealth of materials for the social historian. Matters relating to colonial religion, health and medicine, plantation management, travel, and postal customs emerge from the letters as does the story of strong men and women wrestling the best life possible in a difficult land.

The after-effects of the American Revolution reflected in the *Papers* include a movement toward internal improvements (including canal building), the problems encountered as the Anglican Church became the Protestant Episcopal

Church, the political struggles of the new nation—with the Pettigrews predictably Federalist—and the flourishing of educational institutions. The Pettigrew sons went to the newly founded University of North Carolina, where the father's concern for his sons' morals was reinforced by their reports of life in Chapel Hill in the 1790s. "Cursing & swearing is carried on here to the greatest perfection" with the students preferring Thomas Paine's deistic *Age of Reason* "to all the books that were ever wrote since the creation of the World." Other impediments to learning included a dancing school, six or more boys to a room, a scarcity of books and beds, poor food, and "innumerable" chinchies.

Pettigrew shortly withdrew his sons from the university only to have his oldest son die, perhaps of one of the innumerable fever epidemics that regularly swept the region. Ebenezer then joined his father on his increased holdings in Tyrrel County south of the Albemarle Sound, and they engaged in the growing of rice and wheat. Ebenezer applied himself to correspondence with his friends and the development of machinery and buildings to handle the culture and harvest of the plantation products. Ebenezer's marriage to his cousin provided him with an opportunity to build a large new home and furnish it with the best that could be provided by his New York factor. His correspondence with his wife, who spent the winters with her family in New Bern, shows him to be a kindly master, husband, and father. His devotion to his lands and his family frequently tore him emotionally between his desires to have his loved ones with him and his concern for their health and isolation at his home near Lake Phelps.

Above all, the excellently edited and indexed *Pettigrew Papers* reveal the difficulties that had to be surmounted by even the most prosperous Americans of the early national period. In addition to the intimate details of life in the Albemarle Sound region, student life at the early University of North Carolina and at Princeton is related in the letters of children, relatives, and friends. Views are provided of Haiti, New York, Philadelphia, Baltimore, and Norfolk. The difficulties of transacting business at long distance, with poor mail service, no dependable way to transmit money or goods, and inordi-

nate time consumed by even the most simple of transactions, make one marvel at the determination and abilities of the Pettigrews and of those like them who not only survived but prospered. Volume 1 ends in December 1818, significantly, with the purchase of a slave. Succeeding volumes from the North Carolina Department of Archives and History (which, incidentally, has provided this monumental volume for "the actual cost of printing only") should provide further valuable insight into the Pettigrew family and the region, state, and nation in which they lived.

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Wake Forest University

DOUGLAS SLOAN, *The Scottish Enlightenment and the American College Ideal*. (Teachers College Studies in Education.) [New York:] Teachers College Press, Teachers College, Columbia University. 1971. Pp. xi, 298. \$10.00.

In this mistitled collection of seven essays Douglas Sloan has limited himself to the "thoughts and careers of representative individual educators who were related to the [Presbyterian] academy and early Princeton traditions" instead of appraising the "full impact of Scotland upon the American college." His book is the weaker for it, since most of his subjects are well known (as his excellent bibliographical essay shows), and his presentation of them is seriously flawed.

The basic problem is that he has tried to demonstrate that eighteenth-century American higher education was influenced by the universities of Scotland and the ideas of Scottish intellectuals—a proposition whose meaning is far from clear (if not nebulous) and the logical conditions for proving which are exacting (if not impossible). (See Quentin Skinner, "The Limits of Historical Explanation," *Philosophy*, 58 [July 1966]: 199–215.) Mr. Sloan has neither clarified the one nor satisfied the other, and accordingly his text is fraught with "maybe," "probably," and "seems to have been." When evidence for the influence of or upon his subject peters out, he retreats to lists of the man's former students, who in later life "seem to have" borne the intellectual characteristics chosen as "representative." Because of the slippery

nature of what he is trying to prove, he is effectively forced to accept any and all suggestions of connection between his subjects to such an extent that he never dismisses or discounts evidence even after warning of its possible bias or weakness. In short, the author would have been well advised to heed Hume's warning that mere contiguity in time and space (or similarity) is not sufficient evidence of cause and effect (or "influence").

The book is further marred by an egregious Whiggism. According to Mr. Sloan the Scottish universities were consistently "modern" and "progressive"—which is usually equated with "public service," "utilitarianism," or "reform"—and this in spite of three "watershed" reforms that were never executed. Mr. Sloan seems to have written from the liberal-progressive belief that the burden of proof for change is on conservatives, not reformers, a position that is least valid in education whose very responsibility is the preservation of social ideals.

JAMES AXTELL
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ROBERT ZEMSKY. *Merchants, Farmers, and River Gods: An Essay on Eighteenth-Century American Politics*. Boston: Gambit, 1971. Pp. xiii, 361. \$10.00.

A blend of political history and historical political science, this book discusses Massachusetts politics between 1730 and 1755, using an analysis of legislative decision making as a focal point. Zemsky finds that by comparison with its modern counterparts the Massachusetts Assembly's organization was primitive, with no standing committees, no seniority system, few rules, and debates that actually influenced how members voted. Decision making in this body was orderly not because it was institutionalized, but because the committees that drafted legislation and the debates preceding enactment were dominated by a stable group of leaders—distinguished by social status rather than legislative experience and by the related ability to acquire the contacts, the calculated mode of behavior, the understanding of patronage politics, and the pragmatic approach that united professional politicians. Potential leaders, usually men from distinguished fami-

lies, representatives of coastal towns, or graduates of colonial colleges, emerged upon first appearance in the house, while others, once identified as back-benchers, remained without substantial power throughout their legislative careers.

Such a system worked because the central government concerned itself with defense, justice, taxation, currency manipulation, and little else. The agrarian majority in Massachusetts tolerated the political dominance and officeholding monopoly of upper-class leaders in return for a narrowly defined public policy that served its interests. Although deference to social status influenced electoral behavior as well as legislative leadership, politics remained responsive because professional politicians recognized the limits of deference and took popular positions on sensitive issues in order to preserve their autonomy on other measures. By analyzing the relationships among legislators in the house and between legislators and their constituencies this study adds much to our understanding of colonial political processes.

Zemsky is penetrating as a historical political scientist, but he is less successful at more traditional political history. His interpretations are conventional, rely heavily on standard sources, and, moreover, are flawed with minor errors. Many of the book's weaknesses, however, stem from a major organizational problem. Like many historians who use extensive quantitative sources, the author felt constrained to restrict his presentation of statistical evidence in the interest of historical artistry and has relegated the numbers to an appendix. This decision is counterproductive because statistical analyses provide the most original and suggestive material in the study and the most concise means of supporting major interpretations. A series of biographical sketches provides a partial substitute for the absent quantitative evidence, but they are lengthy, repetitious, and digressive, creating an impression of padding. In marked contrast, the statistical appendix is clear and forceful. Historians should profit from Zemsky's conclusions about political behavior and his methodological discussions, but it is hoped that they will avoid his method of presentation.

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RICHARD HOFSTADTER. *America at 1750: A Social Portrait*. New York: Alfred A. Knopf. 1971. Pp. xvi, 293, xiii. \$6.95.

This book was not complete when Richard Hofstadter died in October 1970. What appears under the title *America at 1750* is in fact an unfinished section of a volume intended to be the first of three on the history of the United States from around 1750 to the recent past. In a prospectus written for his publisher Hofstadter explained that each volume would run to around 500,000 words and would cover a period of around seventy-five years. The decision to publish this fragment, apparently made after Hofstadter's death, was sound, for although the work is incomplete it has unity, and it has what one would expect of its author—fresh insights and literary grace.

In substance the book is a social history of the thirteen continental colonies at 1750: there are chapters on population and immigration, white servitude, the slave trade, black slavery, the middle class, the churches, the Great Awakening, and the effect of the Awakening on the churches and the larger society. These are all familiar subjects in the social history of early America, and much of what is said about them is also familiar. For example, in his discussion of the colonial population, Hofstadter emphasized the connection of immigration to economic growth. In his description of the organization of the slave trade he repeated Philip Curtin's findings that in the long history of the trade the territory of the United States received slightly less than five per cent of all slaves brought to the New World. And, to give one other example of familiar material in the book, the discussion of the Great Awakening makes a good deal of lay-clerical conflict, itineracy, and the separation of churches under revivalistic strains.

Yet, the book has its own freshness, in part because of Hofstadter's conception of social history. There is throughout the book a clear, though largely unstated, concern for social psychology. The social history that the book tells is the history of attitudes, feelings, values, and the psychology of groups as much as it is the description of the "objective" conditions of society, such as numbers of immigrants, rates of economic growth, amounts of property held, and class organization. Consider, for example,

the long-standing assumption of colonial historians that middle-class values pervaded all classes of white society. Hofstadter accepted that assumption—both upper and lower classes looked to the middle, he wrote, because the middle class was large, assertive, and energetic. If this is one of the "facts" of social history Hofstadter's insight into the way the upper class was recruited from the middle, which carried its bourgeois standards upward, is important and suggestive.

The emphasis on church history as a means of revealing social development is also interesting. The methods of analysis in the book are not new here—much of the recent writing on churches and revivalism is replete with a sense of the connections between religion and politics and the coming of the American Revolution. And indeed, Hofstadter in the last three chapters of *America at 1750* resorted to biographical and narrative techniques that are reminiscent of those in his *The American Political Tradition* (1948). There are, for example, shrewd assessments of the revivalists—the portraits of Jonathan Edwards and Gilbert Tennent are vivid, and the accounts of George Whitefield and James Davenport are remarkably sensitive and gentle.

In these vignettes, and in the narrative, the book provides a valuable analysis of the effects of the Awakening on society and in particular the meaning of the event for civic life. Here Hofstadter looked forward to the American Revolution. What he wrote was fragmentary, but provocative in the best sense. One hopes that other scholars will follow his lead, amplifying and perhaps qualifying his insights. In the process the full value of *America at 1750* should become obvious.

ROBERT MIDDLEKAUFF
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Berkeley

RICHARD BAUMAN. *For the Reputation of Truth: Politics, Religion, and Conflict among the Pennsylvania Quakers, 1750–1800*. Baltimore: Johns Hopkins Press. 1971. Pp. xviii, 258. \$10.00.

Richard Bauman is the most recent in a long line of historians and sociologists who have sought to understand what happens when a group of people who have foresworn worldly

involvement, power, and material gain—in this case the Quakers of Pennsylvania—are obliged to assume power, employ it, and reconcile its uses with their religious ideology. By studying the leaders of Quaker society in eighteenth-century Pennsylvania Bauman is able to define three types of behavioral response “among the politically relevant Quakers”: (1) the “politicians,” who embraced the world, enthusiastically engaged in politics, and accommodated their religious principles accordingly; (2) the “reformers,” who called for a withdrawal from politics and worldly affairs and a return to the inward-looking, self-abnegating life of the First Publishers of the Truth; and (3) the “politiques,” who sought the *via media*, hoping to maintain the faith of the founders by using political power to convert the surrounding society to the Quaker ideal. Bauman follows two generations of historians in arguing that for the first half of the eighteenth century Quaker leaders in Pennsylvania mastered the world of business and politics and accommodated their religious views to this worldly success, but that in the second half of the century, with the pressure of war and internal dissension increasing, the “reformers” began to dominate the Quaker leadership and brought a revitalization of the movement through their re-emphasis of the doctrine of meekness and social service rather than the exercise of political power. Others—such as Rufus Jones, Frederick Tolles, and Sydney James—have been at pains to point this out for many years. Bauman’s contribution consists in clarifying the different responses of Quaker leaders to the time of troubles after 1750, in re-emphasizing that the reformers who counseled a withdrawal from politics were still acting politically since they too were bent on influencing the “setting and pursuit of public goals,” and in reminding us that, as Quakers struggled during and after the American Revolution to find an outlet for their political energies that would not conflict with their religious testimony, they discovered a commitment to the oppressed and disinherited members of their society that has ever since given them an influence in moral stewardship out of all proportion to their numbers.

The author’s attempt to use anthropological theory to explain the several reactions of Quaker leaders to political responsibilities is

not especially fruitful. Interdisciplinary analysis requires more than the frequent use of terms such as “role strain,” “processual analysis,” and “adaptations of belief systems”; and in the end this work gains little by reference to the work of political anthropologists. Historians have long known what Friends themselves so clearly expressed—that for Quakers it was extraordinarily difficult in the strife-filled second half of the eighteenth century to exercise political power while remaining true to the original religious commitment of the Society of Friends.

GARY B. NASH

*University of California,
Los Angeles*

THOMAS FLEMING. *The Man Who Dared the Lightning: A New Look at Benjamin Franklin*. New York: William Morrow and Company, 1971. Pp. x, 532. \$12.50.

In the early 1960s Thomas Fleming’s *Now We Are Enemies* came to my attention; a treatment of the battle of Bunker and Breed’s Hills. It was evident that the author had the ability to write attractively and to hold his readers’ attention. In *The Man Who Dared the Lightning* Fleming gives further indication of his talents as a historical popularizer. Both a novelist and historical interpreter, Fleming has not bound himself by some of the rules of writing and documentation followed by others. On the first page of the text, for example, he sets the tone by describing what happens in all thunderstorms as if he were observing the particular squall during which Franklin performed his electrical experiment with the kite. “From bright June sunshine, the sky above hot, muggy Philadelphia began changing to a sour gloomy gray. . . . Windows slammed, tradesmen shut doors . . . , mothers hastily called children indoors, and idlers vanished from street corners.” As the storm broke, “Franklin asked if everything was ready. William nodded” (p. 3). Or (p. 53), “and then with a small shrewd smile [Franklin] noted. . . .” These points are obviously incapable of documentation, but Fleming uses such devices with surprising effect. Beyond cavil, Fleming’s story is the best written of the many available Franklin books. His novelistic style

gives a good mixture of interest, lightness, humor, and serious, well-documented reporting. Fleming is aware of both recent and older treatments of Franklin's career and uses them well. Fleming occasionally relies on inadequate materials; his coverage of Pennsylvania politics would have been vastly improved by using William S. Hanna, *Benjamin Franklin and Pennsylvania Politics* (1964) rather than the source listed in Fleming's bibliography.

There are a few minor errors, as where Fleming refers (p. 14) to Paxton and Donegal "townships" in Pennsylvania. According to James Hutson, a former assistant editor of the *Franklin Papers*, they were not "townships" but counties. One finds an occasional questionable emphasis, as when Fleming indicates that by the time of the Declaration of Independence, Franklin had turned his back on "founding a Western colony" (p. 329). Then later Fleming tells us that Franklin continued his speculative interests well into the war years, and the records indicate that as late as 1788, in a letter to Charles Thomson, Franklin still sought a western tract.

Since it appeared, Carl Van Doren's *Benjamin Franklin* (1964) has been the standard, broad treatment. Less encyclopedic by design, Fleming's study omits much of Franklin's early life and lays less stress on his subject's multifaceted interests, which Van Doren was at such pains to cover thoroughly. Save in a few areas, *The Man Who Dared the Lightning* may now well supersede Van Doren's work as the most widely used biography of Franklin's career.

CECIL B. CURREY
University of South Florida

this disappointing volume were not able to devote as much time to the editing of the work as they did to its compilation, for the former does not match the latter. The introduction is too brief to be satisfactory, and it contains inconsistencies that could have been resolved as well as a glaring typographical error on page xvii that certainly should have been caught and corrected. A seventeen-page section of identifications of persons and places at the end of the book is some help. In the absence of a substantial narrative and analytical introduction, this publication might at least have been the occasion for a comprehensive bibliography of the North Carolina Regulator movement but instead only a very inadequate two-page section on sources is offered. Fortunately, there is an index. All in all, though, this work is an inferior piece of scholarly editing. But why should this be so when both the publisher and the editors, who are well qualified for the task, are capable of much better? The answer—undue haste—is revealed in the foreword. The volume was rushed through to completion in less than a year and a half in order to be published "to coincide with the observance of the two-hundredth anniversary of the Battle of Alamance" (May 16, 1771), the climactic event of the Regulator movement. The objective is a laudable one, but Alamance and the Regulator movement as a whole would have been better memorialized by delaying publication beyond the anniversary in order to produce a scholarly work worthy of the importance of the historical event.

RICHARD MAXWELL BROWN
College of William and Mary

WILLIAM S. POWELL *et al.*, editors. *The Regulators in North Carolina: A Documentary History, 1759-1776*. Raleigh: State Department of Archives and History. 1971. Pp. xxxix, 626. \$12.50.

The chief virtue of this bulky volume is that it brings together for convenient reference in one book many significant documents on the North Carolina Regulator movement that were hitherto accessible in various volumes of *The Colonial Records of North Carolina* (1886-90), in the British Public Record Office, and elsewhere. It is unfortunate that the compilers of

LOUIS DE VORSEY, JR., edited and with an introduction by. *De Brahm's Report of the General Survey in the Southern District of North America*. (Tricentennial Edition, Number 3.) Columbia: University of South Carolina Press for the South Carolina Tricentennial Commission. 1971. Pp. xvi, 325. \$12.95.

This is a splendid work: a little-known report by a remarkable cartographer and observer, edited with scholarly depth and sensitivity and printed in a handsome and sturdy form. The author of the report, William Gerard De Brahm, was a German immigrant who became

surveyor-general of South Carolina, Georgia, and finally, following the Peace of Paris in 1763, surveyor-general for the entire Southern District of North America. He was erudite to the point of pedantry, self-assured to the point of arrogance. He quarreled with colonial governors over his authority; he built forts in the wilderness and fortifications in the cities; he proposed grandiose engineering works that would have bankrupted the tiny English colonies. Nevertheless, he was a superior man who deserved the confidence placed in him by King George III. His contributions are several. He was, as one expert has put it, "the first strictly scientific cartographical expert to practice his art in the Carolinas." His maps, plans, and surveys of the little-known region of Florida acquired by the English Crown in 1763 provided the detailed knowledge necessary for intelligent planning for the area by the English government. Second, De Brahm's description of the natural history of the region and the potential uses to which its resources could be put were a needed corrective to the harsh and unscientific judgments of previous observers. One wonders today at the inability of early travelers in Florida (and in the Southwest, for that matter) to see through the least attractive features of these areas to their potential values. De Brahm was one of those able to look beyond the barren sandy beaches and swarming mosquitoes to the rich storehouse that Florida was. Yet even De Brahm concluded that "I cannot expect my eight years Experience on sandy Soil should be sufficient to out-do the . . . many thousand years entertained Notion" against such a soil. Third, De Brahm was an acute observer of Indian life and not only wrote perceptive comments about the Indians of the Southeast but included a "Compendium of the Cherokee Tongue in English." His surveys of South Carolina, Georgia, and East Florida published here omit only his "Ephemeris" or daily record of weather conditions during certain years, and certain mathematical computations and tables. Twenty-nine maps and sketches demonstrate his skill as a draftsman and cartographer. A solid fifty-seven-page introduction, based on exhaustively mined manuscript sources in England and America, makes the book not merely an edited document but an account of De Brahm in America. However unlike his fellow Ameri-

cans, whether for his specialized calling or even for his mystical writings, De Brahm's story is a fascinating one never before told in such detail. De Vorsey has himself realized his modestly stated hope that the book will lead to a fuller discovery and appreciation of De Brahm's career in America.

WILCOMB E. WASHBURN
Smithsonian Institution

DAVID JOHN JEREMY, editor. *Henry Wansey and His American Journal, 1794*. (Memoirs of the American Philosophical Society, Volume 82.) Philadelphia: the Society. 1970. Pp. xvii, 186. \$7.00.

Progressive Wiltshire clothier, rational Dissenter, Whig, antiquarian, and avid traveler, Henry Wansey was, as a friend said, "always on the fidget." He made the most of a two-month visit to the United States in the summer of 1794, reading about the country beforehand, posing questions to answer, and keeping a careful diary as he traveled from Halifax to Philadelphia. His powers of observation were aided by a fair and open mind. His findings, in the form of an unpolished diary, were published in 1796, followed by a second edition in 1798.

Wansey's business interests prompted his trip, and the principal value of his *Journal* lies in its description of the American textile industry. He concluded about Americans that, "with all their improvements, they must yet for a long time come to John Bull for his cloth, for at least half a century to come." The *Journal* also includes information and commentary on, among other things, ocean travel, the American landscape, commodity and land prices, pronunciation, climate, public figures, and prospects for the country.

Although often cited, the *Journal* was not reprinted until David Jeremy, curator of the Merrimack Valley Textile Museum, produced this incontestably definitive edition. The lengthy introduction acquaints the reader thoroughly with the man through whose eyes he sees the country. The editor has endeavored, with considerable success, to trace every reference in the text. His footnotes correct, explain, and amplify, sometimes unnecessarily. The text, containing perhaps 60,000 words, appears crowded out by the footnotes. There are also

maps, illustrations, a genealogy, bibliography, and index. The reader may feel he has to fight his way through all these aids to find the *Journal*. Nonetheless, by his exhaustive efforts, Mr. Jeremy, with the American Philosophical Society, has made this valuable account not only available, but presented it in a form immensely more useful than the original edition.

JOHN BORDEN ARMSTRONG
Boston University

PETER P. HILL. *William Vans Murray, Federalist Diplomat: The Shaping of Peace with France, 1797-1801*. [Syracuse:] Syracuse University Press. 1971. Pp. ix, 241. \$8.50.

This book serves a dual purpose: it provides the first biographical study of William Vans Murray, a Federalist congressman and diplomat from Maryland's Eastern Shore, and a re-examination of the politics and diplomacy of the Quasi-War with France. Murray, who left insufficient material for a full-scale biography, played an important role in President John Adams's search for an honorable reconciliation with France. Peter P. Hill has used Murray's career as a vehicle for an account of that search.

As the American minister to the Netherlands, Murray became the key figure in the secret diplomacy that gave Adams the assurances he required before he would send his second diplomatic mission to Paris, this time to negotiate successfully an end to hostilities that had erupted into undeclared naval war after the XYZ Affair. Murray also served effectively as a member of the three-man commission that made the peace convention with the new French government of Napoleon Bonaparte and, alone, ably represented his country in the difficult exchange of ratifications at Paris before his recall by President Jefferson in 1801.

In a volume published in 1966 Alexander DeConde considered the politics and diplomacy of the Quasi-War, including Murray's role. Although Hill's work does not supplant DeConde's more extensive study of that subject, it is a useful supplement. By Hill's careful examination of Adams's motives and conduct, in which Hill draws upon Stephen G. Kurtz's observations about naval preparedness as an adjunct to Adams's diplomacy, the author adds

convincing touches to DeConde's portrayal of the president. By Hill's analysis of Murray's life and career, including his association with the Adams family, before President Washington appointed him minister to The Hague in 1797, Hill also helps to explain why Adams had such confidence in Murray as an agent of his policy of peace. In sum, in his biographical treatment of Murray, Hill has furnished a well-researched and well-written case study of one of those "Adams Federalists," as Manning J. Dauer called them, upon whom the president relied as he doggedly pursued his "middle course" amid partisan passions that could have driven him to the side of Britain or of France.

ARTHUR A. RICHMOND III
U.S. Naval Academy

HASKELL M. MONROE, JR. and JAMES T. MCINTOSH, editors. *The Papers of Jefferson Davis*. Volume 1, 1808-1840. (Supported by the William Stamps Farish Fund. Sponsored by William Marsh Rice University and the Jefferson Davis Association.) Baton Rouge: Louisiana State University Press. 1971. Pp. xvi, 594. \$15.00.

To students of the Civil War era, few of the major editorial enterprises now under way are of as much interest as the papers of Jefferson Davis. Hitherto the only comprehensive edition of the works of the Confederate president has been *Jefferson Davis: Constitutionalist*, the ten-volume compilation edited by Dunbar Rowland in 1923. Though still valuable, Rowland's work does not measure up to modern standards of historical editing, and since its publication large numbers of new Davis letters have been discovered, notably those edited by Hudson Strode in *Jefferson Davis: Intimate Letters* (1966). Increasingly, historians have felt the need for a complete, accurate compilation that will permit them to study the complex mind and the troubled heart of one of the most puzzling figures in American history.

The publication of the first volume of *The Papers of Jefferson Davis* gives reason to hope that the need will be met. Though it covers only the thinly documented period of Davis's childhood, his education at Transylvania and West Point, his military experiences in the West, his tragic first marriage, and his resignation from the army, this installment of what is

projected as a twenty to twenty-five volume series includes hundreds of hitherto unpublished Davis documents. Just how this new material will affect interpretations of Davis's career is not easy to predict. The hortatory introductory remarks in this volume by Bruce Catton and Frank E. Vandiver make no attempt to evaluate the documents; and the editors, Haskell M. Monroe, Jr. and James T. McIntosh, have limited themselves to a brief discussion of technical problems of editorial method.

Even without a proper introduction, the documents themselves call for rethinking of problems in Davis's early career. Why was this Mississippi youth at the age of nine sent away to a Catholic school in Kentucky "without my mother's knowledge or consent"? Why did this boy, educated in the North at the nation's expense, so early espouse Southern and sectional sentiments? As a student at West Point he spoke scornfully of "the Yankee part of the corps," who were not "such associates as I would at present select." During the Nullification crisis he was prepared to resign his army commission "rather than be employed in the subjugation or coercion of a State of the Union." How much of Davis's success or failure as Confederate president was determined by the friends and enemies he made as a young officer in the West? As early as 1835 he became strongly attached to Lucius B. Northrop, whom Davis praised as "a very active and Efficient Officer"; thirty years later he was still trying to defend the incompetent Northrop, whom he had insisted upon naming commissary-general of the Confederacy.

If the documents in this first volume illuminate important facets of Davis's personality, the elaborate editorial apparatus that accompanies them resolves a number of disputed questions about Davis's early life. For instance, the editors seem to have settled the long controversy about the date of Davis's birth, which the Confederate president himself remembered incorrectly. Similarly they establish that, contrary to popular tradition, it was "most unlikely" that Davis had any part in the fighting during the Black Hawk War. In a courteous, unobtrusive way they have corrected dozens of errors, large and small, in the previously published versions of Davis's letters. Giving the source of each document reproduced, they have wherever

possible identified every person, place, and incident mentioned. A full chronology of Davis's early life and an elaborate Davis genealogy prepared by Kirk Bentley Barb add to the usefulness of this volume.

Just because this first installment of Davis's papers is so valuable, one could wish that it were more so. Unfortunately most of the 535 documents here reproduced with such loving care have not the slightest biographical or historical significance. Hundreds of the entries consist of monthly reports on the conduct of West Point cadets, of post returns from all the forts where Davis was stationed, of pay vouchers and registers of payments to officers, and of other similar, formal documents that should, at most, have been calendared. Most of the letters addressed to Davis in this volume deserve the same treatment; they consist largely of routine acknowledgments of correspondence by war department officials. Indeed, it would probably have been advisable to calendar most of the eighty letters by Davis himself, about two-thirds of which are brief routine notes transacting army business. It is hard to see how any future historian or biographer will ever need such documents as Davis's letter to William B. Lewis, dated September 27, 1833, which reads in full: "Herewith I have the honor to transmit to you an account current with vouchers covering the amount of funds acknowledged to be the U. States in my last recruitg. account."

The editorial apparatus attached to these letters is also unnecessarily burdensome. Of course it is useful to have every person mentioned in these pages identified, but is it necessary to have a four-hundred-word biographical sketch of Winfield Scott or a two-hundred-word précis of Martin Van Buren's career? The editors of *The Papers of Jefferson Davis* surely do not need to summarize the *Dictionary of American Biography*. Full identification of minor and obscure persons is more defensible, but too often Monroe and McIntosh are excessively detailed. For instance, the Indian trader Peter Pauquette, incidentally mentioned only once in this volume, rates a three-hundred-word footnote, bolstered by the citation of no fewer than nine sources.

Furthermore, the pages of this volume are cluttered by excessive notes. Not content with identifying each person when he is first men-

tioned, Monroe and McIntosh drop a footnote referring back to this biographical sketch when the same name appears in any subsequent document. Thus Peter Hagner, the third auditor of the treasury who acknowledged many of Davis's reports as quartermaster, is identified in footnotes no fewer than twenty-two times. Since this volume has an admirably complete and accurate index, there is no reason for such a superfluity of cross-references.

Behind these criticisms lies a sense of urgency on the part of the practicing historian, who sees all too many of our major editorial enterprises bogged down in trivia and enmeshed in technicalities. If publication of scholarly editions of the papers of notable Americans is to be completed within the lifetime of any subscriber to this issue of the *American Historical Review*, the editors must begin to move with greater speed and with greater selectivity.

DAVID HERBERT DONALD
Johns Hopkins University

MICHAEL P. CONZEN. *Frontier Farming in an Urban Shadow: The Influence of Madison's Proximity on the Agricultural Development of Blooming Grove, Wisconsin*. (Logmark Editions.) Madison: State Historical Society of Wisconsin for the Department of History, University of Wisconsin. 1971. Pp. xviii, 235. \$10.00.

Blooming Grove, Wisconsin, is the township immediately east of Madison, and this book assesses certain aspects of the township's agricultural history between 1835 and 1880. As the title indicates, the author investigates the growing impact of the capital city on the farming township. He does so through a quantitative analysis of major economic factors from the time the federal government first sold the land until the last manuscript census was available. He considers such problems as land ownership and use, the types of people who settled and stayed on the land, the size and value of farms, kinds of production, and marketing. Not surprisingly Conzen finds that Madison consistently exerted significant influence on Blooming Grove.

The book reflects meticulous research, with numerous maps, tables, and graphs amplifying the text. Throughout the book the author's research design dominates the material, and the

mechanics of presentation protrude unnecessarily. There is no question that the study proves its points. It tests various theses related to the data and makes appropriate comparisons with agricultural developments in neighboring areas. In this way it makes a contribution in establishing the particularities of economic growth in a limited region. Against such concrete information further comparisons may be made.

My reservations about the book are two. The author assumes that the reader is completely familiar with the extensive technical jargon associated with Conzen's quantitative approach to historical analysis, and he does not bother to make logical explanations where they are needed. For instance, only on the next-to-last page does he explain "cohort analysis," a model he uses frequently in the book. More fundamental is the fact that for all the specificity of data and scientific treatment of economic development, the reader comes away with scant sense of the human beings who labored the earth of Blooming Grove. The study has scientific sterility, to be sure, but it evokes too little of the past.

WALTER RUNDELL, JR.
University of Maryland

ERLING A. ERICKSON. *Banking in Frontier Iowa, 1836-1865*. Ames: Iowa State University Press. 1971. Pp. x, 183.

For almost three decades the people of Iowa struggled to create a safe banking system, defined by the author as "a sound paper currency." From the beginning of white settlement the attitude prevailed that credit was bad for farmers and honest men in general; banks, the agencies of credit, should not be tolerated. Four recognizably distinct periods with differing systems followed, including a period of eleven years in which all "banks of issue" were prohibited. The soundest paper currency was obtained after the establishment of the national bank system in 1863. Failure of earlier systems and the necessity for change resulted largely from the increasing complexity of the Iowa economy, a widespread demand for more rapid development, and the growth of a national rather than local market for both agricultural produce and purchased commodities.

In a detailed, clearly organized, and well-

written monograph Erickson presents the story in six chapters, followed by a brief, well-considered "overview" in which he relates the changes to state party politics: "Old school" Democrats (by one of the paradoxes of American historical vocabulary they are the "radicals") opposed all banks; Whigs favored an elastic currency regulated by a national bank; conservative Democrats stood between them. His general approach and conclusions are for the most part in line with the work of Bray Hammond rather than A. M. Schlesinger, Jr. and other pro-Jackson historians.

The study is based on extensive research in published sources and secondary works and an astonishing amount of manuscript material. The author suggests that his generalizations "are probably true for a number of other frontier states." This may well be so, but the scene is far different from that shown, for example, by William H. Brantley for frontier Alabama. The genre of state banking history, although well established and perhaps somewhat rigid, is far from exhausted. Interesting illustrations, excellent footnotes, a series of appendixes, a bibliography of over three hundred items, and a dependable index complete this valuable work.

HARRY R. STEVENS
Ohio University

DONALD JACKSON and MARY LEE SPENCE, editors. *The Expeditions of John Charles Frémont. Volume 1, Travels from 1838 to 1844*, and map portfolio. Urbana: University of Illinois Press. 1970. Pp. xlv, 854; map portfolio, pp. 16, 5 maps. \$22.50 the set; map portfolio only, \$10.00.

Anyone who ever read an American history textbook should be remotely familiar with the glamorous John Charles Frémont, and most of those who have gone beyond the survey level can appreciate his contributions as an explorer and map maker at a time when thousands of migrants, promoters, and politicians were hungry for information about the West. With the possible exception of the president of the United States, few Americans were better known in the early 1840s. But like most men dramatically elevated to the status of a hero Frémont turned out to be a mere mortal with feet of clay. His rise to fame following the 1842

expedition to South Pass and the one a year later to Oregon and California was meteoric, but almost everything attempted after 1847 in the fields of finance, exploration, politics, and military leadership seemed to end in disappointment, failure, or outright disaster.

The first of the projected three volumes of Frémont's expeditions probably will not make the best-seller list, but any collection of Western Americana will be incomplete without this very attractive publication. Even more handsome is the accompanying *Portfolio*, which contains the famous 1843 map by Joseph N. Nicollet of the hydrographical basin of the upper Missouri. The most significant of the remaining four facsimile reproductions is the Frémont map of 1845. This detailed work was compiled with the help of Charles Pruess and soon became the base for subsequent explorers to expand the boundaries of cartographic knowledge of the Far West.

Volume 1 in the current series presents a total of 137 thoroughly annotated documents, most of which are hitherto unpublished letters. Interspersed with these are extensive selections from Frémont's *Memoirs*, which only carry the story of his life to 1847 and his ill-fated appointment as governor of California by Commodore Robert F. Stockton. The principal events of his remaining forty-three years were faithfully chronicled by his adoring Jessie, but the sequel was never published. The well-written biographical essay included by the editors in the introduction to *The Expeditions* is disappointingly sketchy. For however Frémont's character was flawed by vanity, his monumental successes and failures have never made dull reading.

The various documents in the first volume reveal the explorer's wide range of scientific knowledge, and the carefully selected excerpts from the *Memoirs* are still fascinating to read. In comparison with the turgid style that characterized much exploration literature of the early nineteenth century Frémont's writing remains a model of clarity and smoothness. On the other hand, the many "tables of meteorological observations" are not nearly as exciting to read today as the New York City Telephone Directory. But to have excluded such valuable scientific information from this publication, especially after Frémont's long and painstaking

efforts in collecting it, would have been a serious omission.

W. EUGENE HOLLON
University of Toledo

NANCY NICHOLS BARKER, translated and edited with an introduction by. *The French Legation in Texas. Volume 1, Recognition, Rupture, and Reconciliation.* With a foreword by JOHN CONNALLY. Austin: Texas State Historical Association. 1971. Pp. 357. \$12.00.

In the spring of 1838 at the request of the Texas agent to Britain and France, James Pinckney Henderson, the French Foreign Ministry began consideration of the recognition of the Republic of Texas. In September a young clerk in the French embassy in Washington, known as Alphonse DuBois de Saligny, was instructed to make a tour of inspection in Texas. He arrived in February 1839 and left in May for Europe, recommending strongly that France grant recognition to Texas. A treaty of recognition was signed on September 25, 1839, and the following month Saligny was named chargé d'affaires in Texas. He reached Houston in January 1840 and took up the duties of France's representative in Texas, which he was to manage intermittently until the annexation of Texas to the United States.

This volume is a record of his correspondence from May 1838 to August 1842. A projected second volume will cover the remaining period. The source of the letter is the archives of the French Foreign Ministry: nine bound volumes of political correspondence and one of commercial correspondence. Dr. Nancy Barker, the editor of this work, undertook the no doubt agonizing task of translating these from microfilm copies. A specialist in French history, she is unusually well equipped to explain the European nuances and overtones in the correspondence, and she has done a creditable job of handling the Texas angles, although here and there a few relatively unimportant weaknesses can be found. The two-volume work, when finished, will contain approximately half of the total correspondence. The editor has selected letters and passages from letters that seemed to be the most significant and has included brief abstracts of the omitted portions.

An even greater contribution than her anno-

tated translations, however, is her praiseworthy introduction, which traces the life of Saligny and puts him and the French legation in proper perspective. Until this work relatively little was known about the man who styled himself a count and stalked pompously on and off the stage of Texas history. Dr. Barker shows that he was born plain Jean Pierre Isidore Alphonse Dubois, that he was frequently untrustworthy, that he was something of a rascal, and that his letters and reports from Texas were unreliable.

It is this very lack of reliability that makes one wonder if this monumental work was really worth the doing. I would have much preferred to read a scholarly synthesis by Dr. Barker of Texas-French relations.

The whole effort to produce this and the volume to come appears to be some sort of special cause. The French ambassador personally presented the microfilm to the Austin Public Library; John Connally wrote a nice but needless forward; the volume is handsomely designed and beautifully printed and bound—at a production cost of probably twice the usual; and volume 1 was released with a grand fanfare but, naturally, without index or bibliography.

SEYMOUR V. CONNOR
Texas Tech University

HENRY B. COMSTOCK. *The Iron Horse.* New York: Thomas Y. Crowell Company. 1971. Pp. 228. \$17.50.

A long century ago the sound of the locomotive whistle was one of the more exciting sounds in America. Not only were railroads then playing a vital role in our national development, but more specifically the steam locomotive was wooing youngsters from both farm and town toward a new-found interest in mechanics and the practical arts. *The Iron Horse* carries the history of the steam locomotive in our country from Colonel John Stevens's midget engine, built and run on his Hoboken, New Jersey, estate in 1825, to the 604-ton, 133-foot "Big Boy" built for the Union Pacific in the early months of World War II.

In the century and a third between the first American locomotives and the full acceptance of the diesel, about 180,000 steam locomotives were built in the United States. As the author

traces the early history of railroad motive power he presents much new biographical material about such early engine builders as Peter Cooper, Horatio Allen, John B. Jervis, Isaac Dripps, Matthias W. Baldwin, Henry R. Campbell, William Norris, and Ross Winans. Well before the Civil War locomotive builders had solved the basic mechanical problems of steam locomotion and had added such functional accessories as oil headlights, sand domes, bells, whistles, and cabs. As the growing transportation needs of the nation demanded ever larger locomotives and the introduction of such features as new wheel arrangements, "three-cylinder compounds," and the articulated Mallet, most railroad mechanical departments still were insisting on their own individualistic whims in engine design. Only during the days of federal control in World War I was there any degree of standardization in locomotive design and construction. The basic decision of early diesel manufacturers to standardize their product no doubt hastened the acceptance of this new form of motive power in the mid-twentieth century.

Drawing upon his long experience as a technical illustrator the author has enriched his text with more than two hundred drawings and illustrations. Much of the rich anecdotal material Comstock has included has not previously appeared in the standard secondary histories of the industry. A ten-page glossary of terms is helpful, but the bibliography is very brief and does not even include the valuable 1968 work on locomotives by John H. White, Jr. But the faults in this volume are few. Even though primarily written for the railroad buff *The Iron Horse* can be a valuable volume for any student interested in the technical and industrial history of the United States.

JOHN F. STOVER
Purdue University

MORRIS F. TAYLOR. *First Mail West: Stagecoach Lines on the Santa Fe Trail*. Albuquerque: University of New Mexico Press. 1971. Pp. 253. \$10.00.

The history of the Santa Fe Trail before the Mexican War has long been familiar through the work of the contemporary historian Josiah Gregg and that of subsequent writers who have

drawn upon and supplemented his *Commerce of the Prairies* (1844). It is not generally realized, however, that the Santa Fe Trail continued to be of great importance until 1880 when the services of the Atchison, Topeka and Santa Fe Railroad rendered passenger, freight, and mail service over the old trail obsolete and unprofitable. Professor Taylor has filled a definite gap in knowledge by providing a history of the thirty-year period from 1850 to 1880 during which the Santa Fe Trail continued to be a vital link in the transportation system of the far southwest.

The history of stagecoach and mail service in these years is presented with the easy authority of one who knows his subject as the author unfolds the story of Waldo, Hall and Company, Hockaday and Hall, Hall and Porter, the Santa Fe Stage Company, and finally, Barlow, Sanderson and Company. The material for the work has been gleaned from a great variety of scattered contemporary and primary sources and has been blended with skill to form a connected account.

Among the points made by the author the following may be of interest. In a sense, the Santa Fe Trail may be viewed as a continuation of the National Road. A large number of pioneer Jewish merchants participated in the trade. After William Bent abandoned it in 1849 Bent's Old Fort was for many years used as a stagecoach station.

A good map and a few interesting illustrations are provided. The index and bibliography are excellent. The University of New Mexico Press has furnished a handsome cover and jacket, and both printing and paper are of a quality that matches the research and narrative skill of the author.

HARVEY L. CARTER
Colorado College

H. CRAIG MINER. *The St. Louis-San Francisco Transcontinental Railroad: The Thirty-fifth Parallel Project, 1853-1890*. Lawrence: University Press of Kansas. 1972. Pp. viii, 236. \$8.50.

The purpose of the St. Louis-San Francisco Railroad was to establish a snow-free route to the coast that would have an advantage over other railroads in tapping the vast commerce that was expected to develop between San

Francisco and the Asian countries. The building of a railroad along the thirty-fifth parallel was a project that became involved in many financial, legal, and competitive problems. The project suffered from faulty administration, labor troubles, Indian disputes, and the disruption brought on by the Civil War. Although involved in many construction problems the Frisco eventually became a successful railroad and has become a good investment for the stockholders. The road had several changes in name—the Southwest Branch Railroad, the Southwest Pacific Railroad Company, the Atlantic and Pacific Railroad, the South Pacific Railroad Company, and the St. Louis and San Francisco Railway Company.

Writing the history of some railroads has been difficult because of a lack of records. Professor Miner, as a result of persistent research, found newspapers, government documents, diaries, and corporate records. Some of the material was located in old railroad warehouses and basement vaults of corporate office buildings. The book is illustrated with two maps and sixteen pictures of locomotives and persons connected with the railroad.

JOHN H. KRENKEL
Arizona State University

NORMAN E. TUTOROW. *Leland Stanford: Man of Many Careers*. Menlo Park, Calif.: Pacific Coast Publishers. 1971. Pp. xiii, 317. \$9.95.

Leland Stanford, as the subtitle of this book suggests, had interests other than railroading. But it was in the business of transportation that he made his money; and, like many another nineteenth-century tycoon, he spent in a manner that both interested him and at the same time afforded him a claim either to philanthropy or the advancement of learning. These endeavors, in turn, earned for him sufficient prominence that his adopted State of California accorded him the traditional honor of a place in the U.S. Senate. There was even a brief "Stanford-for-President" boom as a final accolade.

In an effort to describe the usually neglected aspects of Stanford's career the author has compartmentalized Stanford's various enterprises, giving a good deal of attention to some of the lesser-known portions of it. For example, a chap-

ter is devoted to the magnate's role as a country squire: he invested a great deal of money in horseflesh, developed some prizewinning trotters, and gained a name as a breeder of superior animals. In his efforts to learn more about the mechanics of trotting Stanford spent considerable money in setting up a battery of cameras designed to record each motion of a moving horse. As a result he contributed to what would later be the motion picture industry.

Another of the railroad prince's efforts was to capture the title of Wine King of California, and into that enterprise he poured money with a reckless abandon. Some nine million dollars later the gentleman farmer was forced to recognize that money could not buy everything; the quality of his wine fell short of the anticipated goal. He then turned to brandy and in turning it out at a rate of 1,400 gallons a day could claim title to the world's largest distiller of grape brandy.

As a very rich man Stanford followed the path of his nineteenth-century fellows, traveling abroad in search of culture, dabbling in politics, and building mansions befitting of his position, but in founding a university as a memorial to his lost son he gained a peg or two on the others, many of whose names are today relatively obscure. In reviewing his subject's career the author candidly admits that the man "hovered on the edge of greatness, but never quite made it." Agreed, but one has to admit that in spending his money Stanford did a better job than a great many of his contemporaries. And in telling us about it, Professor Tutorow has made a contribution.

ROBERT G. ATHEARN
University of Colorado

History of the Supreme Court of the United States. Volume 6, *Reconstruction and Reunion, 1864-88*. Part 1. By CHARLES FAIRMAN. (The Oliver Wendell Holmes Devise.) New York: Macmillan Company. 1971. Pp. xix, 1540. \$30.00.

This book deserves, indeed requires, a category more than a review. In its 1,500-plus pages Professor Fairman has created the equivalents of a half-dozen ordinary monographs; a useful collective biography of Chief Justice Salmon P. Chase and his brethren; several legal treatises; a substantial documents-case source book

blessed with rich headnotes; and, in the extensive footnoting, a treasury of bibliography, comment, criticism, lore, and wisdom. Yet this is only the first of two parts—i.e., volumes—allotted to Fairman for the Reconstruction period, in the eleven-volume Supreme Court history underwritten by the devise of the late Oliver Wendell Holmes, Jr.

I wish that Fairman had reached print a bit sooner. Literally on the day after I mailed off to a publisher my manuscript on Civil War and Reconstruction constitutional impacts, the review copy of Fairman's *Reconstruction and Reunion* came in. Available earlier, its information and judgments would have greatly eased my way.

This almost-immovable feast will guide and inform generations to come; any serious collection or library worth using will have a copy. Determined readers, blessed with good eyesight and sturdy bookrests, will exploit it as a standard by which to measure others' more modestly conceived efforts. Where else can one find appreciation of the 1867 Bankruptcy Act, or two hundred pages of minutely detailed coverage and analysis of mid-nineteenth-century municipal bond controversies, so long forgotten by other constitutionalists that our best present textbooks in that specialization ignore even the leading case, *Gelpcke v. Dubuque* (1863)? It is essential to understanding of Reconstruction alternatives to note, as Fairman has, that Lincoln's contemporaries were almost as vitally concerned about debt repudiations by Northern communities as about Southern state secessions and restorations; after Appomattox, more concerned. This duality of concerns was so deep among law men at least that the United States Supreme Court reporter saw fit especially to note in the preface to the 1863 *Reports* how, in holding Dubuque to payment, the national Court was preserving essential morality that Iowa had failed to nurture. As Reconstruction matters worked out, this tenacious dualism was to prevent Republicans from attending with full hearts and minds to Southern questions, as Dubuque-like Northern concerns gradually took precedence.

Similarly, it is basic to useful insights into the Supreme Court's Reconstruction history to understand legal-judicial procedures and the technicalities of pleadings, the mysterious sci-

ence by which ostensibly workaday private interests and relationships became transformed through litigation into public law. Professor Fairman, the distinguished biographer of Justice Miller and the worthy contender against Justice Black on the question of whether the Fourteenth Amendment carried national Bill of Rights constraints against states, is one of the few scholars who could have coped with the task. His *Reconstruction and Reunion* is a monumental achievement.

But, the question persists, achievement in what? Constitutional history? If so, then Professor Fairman's definition of the field is the widest-ranging since 1902, when Francis Thorpe, asking "What is a Constitutional History of the United States?" (*Annals of the American Academy of Political and Social Science*, 19 [1902]: 259–65), suggested that its sources included national and state constitutions, derivative statutes, legislative records, and, of course, the "mass of authoritative exposition by the courts." Untroubled by the mountains of printed data included in his free-swinging definition, Thorpe asserted that "the entire testimony bears back to principles of government, few in number and comprehensive in character." And, he concluded, "A constitutional history is the narrative of the apprehension and application of these principles by the American people" (p. 259).

Thorpe's *fin-de-siècle* confidence concerning basic constitutional principles failed to inspire derivative histories of those principles or of their primary expounder, the Supreme Court. During ensuing decades raiders from present-minded political science and law departments on campuses so transformed constitutional inquiries that by 1963 Professor Paul Murphy pleaded in this journal that it was "time to reclaim" the specialty for history and historians ("Time to Reclaim: The Current Challenge of American Constitutional History," 69 [1963–64]: 64–79).

Professor Fairman's *Reconstruction* volume will advance the ongoing reclamation if historians are not misled by its encyclopedic size and comprehensiveness into assuming that the book is constitutional history's final massive marker. Fairman labored to praise constitutional scholarship not to bury it, to augment understanding not to enlarge obsequies, and to loose constitu-

tional history in Clio's many-roomed house not to constrain it.

In some instances Fairman's freedom to include great data masses enriched the whole. Publishing economics may make this the last time that an author, impressed with the passionate quality of scholarship on a point under consideration, felt free to employ, fairly casually, more than a dozen pages for quotations of pertinent contemporary newspaper comment (pp. 343-55). In many others less inclusiveness and far tighter editorial control would have greatly improved the book's quality and utility. In search of rich lodes, one must push through many pages of inappropriate impediments. As examples, Fairman includes descriptions of the Court's seating arrangements; the floor plan of its chambers, statuary, and other decorations; robing-room situation; and members' reading and work habits. The filing systems the Court clerk and marshal developed receive attention. One can defend the unstinted detail Fairman employed on the basis that technical legal procedures are often the heart of substantive constitutionalism, ignored only at peril. But procedure is not trivia.

Fairman's *Reconstruction* sins only in minor terms. Often provocative, insightful, and thoughtful, it marches generally in step with the impressive recent reconsiderations of men and measures of Republicans during the Reconstruction era, yet fails to analyze adequately the aspirations, alternatives, and achievements of the framers and ratifiers of the Thirteenth through Fifteenth Amendments and derivative legislation.

This being so, I wonder why, considering Fairman's policy (p. xix) deliberately to keep references sparse, he troubled to cite Bentley's defective Freedmen's Bureau study and failed to note Kutler's more useful inquiries into Court numbers, *ex parte McCardle*, and Reconstruction politics? That Justice Miller should have understood Negroes because he was a Kentuckian (p. 124) is a quaintly antique suggestion. In light of Fairman's sensitivity to the national Court's bootstrapping role concerning Iowa's remissness in failing to check Dubuque, I wonder at his flat reaction to the high judges' equally adventurous jurisdictional stand concerning Missouri's test oath constitutional requirement (pp. 244-45). I regret the

curiously thin treatment Fairman gave to the Johnson impeachment (pp. 521-27); its constitutional and legal history rates far fuller attention. Fairman's decision was unwise to wait for pages 334-43 before offering lucid discussion of Reconstruction options and alternatives available in the 1860s to national policy makers in light of existing institutions, resources, and limitations—constitutional and otherwise. That sage analysis deserved front-of-the-book space.

But an imperial adventure deserves consideration on its terms not by a carper's tight horizons. I welcome, though I am troubled by, Fairman's *Reconstruction and Reunion* and wait eagerly its promised companion "part."

HAROLD M. HYMAN
Rice University

ROBERT HIGGS. *The Transformation of the American Economy, 1865-1914*. (The Wiley Series in American Economic History.) New York: John Wiley and Sons. 1971. Pp. xv, 143. \$5.95.

In recent years economic historians, old and new, have improved in substantial ways our knowledge of the early nineteenth-century American economy. Most of this research has been guided by an interest in explaining the origins of rapid industrialization and of the growth of income that was one of the most attractive aspects of that complex historical process. When and why, scholars have repeatedly asked, did that process begin? So intent have they been on answering these questions that they have given little attention to later, equally important economic developments. As a result, we now know more about the workings of the economy in the twenty years before the Civil War than in the several decades that followed 1865. The appearance of Robert Higgs's study is therefore welcome, especially insofar as the volume is likely to arouse new interest in the modern American economy.

In this slim book the author gives us a series of discrete essays in search of a synthesis. After explaining his analytical framework, which is based on neoclassical economic theory (with a few special flourishes), Higgs attacks the problems of relating urbanization, health care, invention, and information exchange to the country's rapid growth. There is a brief digression on the antebellum years, a section in which the author seems to follow Stuart Bruch-

ey's treatment of the importance of property rights without, curiously, mentioning Bruchey. Higgs also explores the increased productivity and multiplying problems of the farmer, and he concludes with an examination of America's unequal distribution of wealth and welfare.

The book is well written and is clearly intended for historians, not economists. Any reasonably intelligent reader will encounter no problems in following the author's analyses. Indeed much of the book is elementary and familiar; historians will not be too surprised when they read about the end of the frontier and the rise of the city in the years 1865-1914. More novel are the author's comments on the sources of increased productivity and the economic significance of improved health care and of new means of transmitting information. These are subjects that will doubtless receive considerable attention in the future, and Higgs will be responsible for the direction this research takes.

For the present, however, his treatment of these and other related problems is mildly disappointing. All too often the author follows an elaborate explanation of hypothetical consequences with an admission that the evidence needed to test his ideas does not exist. In other cases the available data seem to indicate that the most important effects fell outside of the book's time period. Instead of this kind of particularistic probing, what we obviously need is a synthesis—something comparable to Douglass North's volume on the period 1790-1860; but all that Higgs gives us is a theory grounded in the assumption that America had a relatively efficient market economy and that static theory therefore provides the best means of analyzing change. Having admitted that the market worked rather well, however, we still need to know what the major determinants and long-run consequences of economic growth were. Numerous scholars have suggested that some dramatic changes took place in the economy around the end of the nineteenth century. Unfortunately Higgs has not addressed himself to the questions they have raised, and we can only hope that his book will be followed by another that will apply the author's substantial talents to this more challenging task.

LOUIS GALAMBOS
Johns Hopkins University

SHERRY H. OLSON. *The Depletion Myth: A History of Railroad Use of Timber*. Cambridge, Mass.: Harvard University Press. 1971. Pp. xvi, 228. \$9.00.

One hundred years ago and for many decades thereafter presumably knowledgeable persons predicted an imminent and inevitable "timber famine" in America. In one of her pungent sentences Mrs. Olson states that "the crisis never came to pass" (p. 2). The precise reasons why it did not come to pass have never been this clearly analyzed. The author, who teaches in the department of geography and environmental engineering at Johns Hopkins University, hews straight to the mark and clears away hoary misconceptions and confused thinking based on plain ignorance of the facts. She says of her analysis that "it differs critically from the classic fairy tale of American history and the traditional foresters' version of forest history" (p. 7). That it does.

Since the railroads once consumed from one-fifth to one-fourth of the nation's annual timber production, she presents data concerning railroad use of timber as a case history to show exactly what happened and why and how expert and popular opinions concerning timber depletion led to misdirected and costly public and private policies. She reviews the whole gamut of forest policy, sparing not even the revered Gifford Pinchot, whose advice concerning "good forestry" was too dogmatic and based on the European experience with silviculture and fire protection. Far more attention should have been given to wood utilization for, "Use alone can create value." Conservationists erred in confusing the growing of trees with the economic facts of supply and demand.

The incisive and valid conclusions reached are backed by impeccable analysis of abundant data, well documented. This diminutive but significant book contains eight illustrations, twenty-six figures (maps and graphs), and five tables. It offers a fresh approach to an important subject, and the findings can offer guidelines for anyone concerned with current problems of resource management, whether animal, vegetable, mineral, soil, or water. It is hoped that the book will become available in paperback form at a much reduced price.

FRED W. KOHLMAYER
Illinois State University

ALBRO MARTIN. *Enterprise Denied: Origins of the Decline of American Railroads, 1897-1917*. New York: Columbia University Press. 1971. Pp. xiv, 402. \$10.95.

Professor Martin believes that the physical and financial demolition of the American railroad system was begun in the years 1906-17 by "archaic Progressives" who were in turn manipulated by organized labor and industrial and agricultural shippers. He does not mince words: "What I have to relate is a story of the most intense frustration of the human spirit; of the brutal substitution of petty consistency for a sensible pragmatism; of the unconscionable elevation, by the government of a republic, of one set of interests over another and over the general welfare; and of a self-serving and all but cowardly refusal to face public duty." His heroes are the frustrated managers of such roads as the Pennsylvania, the Illinois Central, and the Santa Fe; his devils are the misguided initiators and inept administrators of public policy during the Roosevelt, Taft, and Wilson administrations.

The thesis of this book is clearly stated: Following the reorganizations of the 1890s, the fundamental need of American railroads was for investment capital to enable them to rebuild and modernize. Among those roads that accounted for "most of the tonnage and passengers, if not the total route mileage," an "advanced technology and an amazingly vital railroad management stood fully ready." But after 1906 public policy became increasingly unresponsive to the needs of these strong, soundly managed roads in that it fomented high labor costs and refused to grant general rate increases. The result was that the flow of investment funds available to the railroads did not keep pace with demands placed upon them. This combination of a cost-price squeeze and a lag in new investment caused profits to disappear after 1911 and paved the way for the eventual collapse of the the system of private management of the railroads. In this scenario the Hepburn Act of 1906 and the Mann-Elkins Act of 1910 appear not as milestones in the government regulation of economic activity but as the gravestones of responsible private enterprise.

The proof of this thesis rests upon two quantitative estimates and a qualitative appraisal of

the public policy options available at the time. Professor Martin begins with a comparison of the total annual output of freight and passenger services ("demands on the system") with the annual net new investment in railroads ("the absolute volume of national resources going to increase the capacity and efficiency of the system") in the base period 1905-07. He then computes, year by year for the period 1898-1915, what the annual net new investment would have been if it had been of the same magnitude in relation to the base period as was transportation output. These estimates lead him to the conclusion that the relationship between the trendlines of railroad transportation output and net annual new investment was "rational" in the 1898-1906 period and "irrational" thereafter. In short there was after 1906 a very large "annual and cumulative deficiency" in the flow of funds into the railroad system.

These calculations are justified in an eleven-page appendix, but it would be a mistake to consider them either the last word on the subject or the bedrock of Professor Martin's case. The major burden of his argument is found in his text, which seeks to prove that public policy makers understood these economic relationships and their implications, had alternatives available to them that would have benefited the railroads and the nation, but consciously chose and implemented policies that had opposite and detrimental effects.

Professor Martin has filed a brief seeking an indictment for conspiracy against the public officials of the Progressive era. He argues his plea with gusto and conviction. To this member of the audience his evidence still remains circumstantial and inferential. If one believes in such conspiracies, one will find Professor Martin's evidence admissible and his conclusions irrefutable. If one does not believe in such conspiracies, one must render a Scotch verdict at best. But one must admire the diligence and skill with which Professor Martin builds his case. He is certainly the most able advocate the railroads have had, then or since.

JAMES P. BAUGHMAN
Harvard University

JOHN B. RAE. *The Road and the Car in American Life*. Cambridge, Mass.: MIT Press. 1971. Pp. xiv, 390. \$12.00.

In an age of often misguided criticism and rejection of industrial society, Professor Rae's study defends the automobile-highway system by looking at its history. On sturdy democratic grounds he rejects the idea of banning cars in Yellowstone even though they take two million tourists there each year with destructive effects. He takes a negative view of user taxes or of adopting highway revenues for other uses (a "breach of faith" with road-users and a form of discriminatory taxation banned by some states). He finds that new highways raise land values, though some people suffer (the depredations of "eminent domain" are not described). "Planned obsolescence" is "an overworked expression"; city congestion existed before the automobile; and the urban freeways take up less street space than conventional roads (despite exaggerated claims of how much of Los Angeles the car consumes). The car has brought a disastrous decay of public mass transport and has accelerated suburbanization. Professor Rae gives a needed argument in favor of the much-maligned suburbs of today. Above all, the gist of his argument is that the automobile-highway system exists because the market society chose it. People want it—here and in other nations.

Having a point of view helps the author to pull together this interesting and clearly written study of the interaction of the mass-produced car and the hard-surfaced, high-speed highway in recent history. Extending his study of 1965 (*The American Automobile*), his first eighty pages summarize familiar historical facts, while the bulk of the book concerns events since 1930. Rae touches briefly upon many topics, such as the role of highways in ancient empires, English and French road-building in the seventeenth and eighteenth centuries, turnpikes, and the eclipse of highways by railroads. Here readers will be reminded of Robert Fogel's more technical argument on the hidden costs of railroads; we see how railroads choked off facilities—good roads, inns, restaurants.

After 1900 people knew how to build roads well; they did not do so. The bicycle mania of the 1880s and 1890s got people out on the primitive dirt highways and created a road lobby (groups like the League of American Wheelmen, described already by Philip Mason). Farm pressure for local market roads and the auto-

mobile brought the real beginnings of state and national legislation. Rural Free Delivery (general by 1900) also helped. Professor Rae traces the history of public highway policy from the first federal act of 1902. Major developments after 1930 were the pioneering "parkways" built by the states, the turnpike revival of the 1950s, the California freeways, and the interstate highways built under the Federal-Aid Act of 1956, which will cover the nation with 41,000 miles of superhighway.

Professor Rae is at his best in economic description and explanation; he is less concerned with the social impact of the new system, though he suggests its dimensions: the life-style based on the car ("Drive-in America"); the elimination of rural-urban distinctions ("Rurban" America); the greater opportunities and rootlessness of motorized Americans. Though he describes the enrichment of rural life there is still more to be said about this "flexible, multiple-use vehicle," this private room on wheels, that has given Americans such cheap personal mobility. Its heritage has been ambiguous.

PETER D'A. JONES
*University of Illinois,
Chicago*

BARBARA W. TUCHMAN. *Stilwell and the American Experience in China, 1911-45*. New York: Macmillan Company. 1971. Pp. xv, 621. \$10.00.

Barbara W. Tuchman has brought her considerable talent and audience to the subject of American-Chinese relations at an important time. This work, like her earlier books on the balance of terror in World War I European diplomacy, will have a wide impact on popular perception of present problems.

Despite, or because of, its broad compass, this mass-market historiography breaks certain rigorous research rules. Mrs. Tuchman does not branch out far from the papers of public and visible men. She has not delved deeply into scholarly, periodical literature nor explored exciting interdisciplinary suggestions about the professional, military aristocracy into which her subject fits. Finally, she is too trusting of secondary materials on many of her period's leaders and events.

Mrs. Tuchman is far too skillful at her genre to dwell on these problems inherent within it.

She culls so much from the sources she uses, paints such a vivid canvas, and fixes so well upon important conclusions. Further research, indeed, would probably not challenge the book's already well-quoted summation, "China was a problem for which there was no American solution."

In the context of these strengths and weaknesses in a book that will have a wide reading, it is important to examine its intent and ask basic interpretive questions about its significance. *Stilwell* is both a biography and a policy study. Its central figure is both a man and an archetype. As Theodore White's 1948 publication of *The Stilwell Papers* had already proven, General "Vinegar" Joe provided more than enough material, observation, participation, and disposition for reporter and biographer. He is a worthy match for the author's talents. She takes hold of the young Stilwell before 1900, his plebe year at West Point, and refuses to let go for nearly half a century through to the four-star general's receipt of his Combat Infantryman Badge a day before his death in October 1946.

Against this Stilwell setting Barbara Tuchman has chosen to trace the general outline of United States-Chinese relations in which the general played a major role as a military liaison officer in the 1940s. In the effort to capture the "American experience" in China in the light of one career, however responsive and representative, the author finds her most serious problems. No individual in either society, not even Stilwell, transcends effectively the period from the onset of Chinese revolution in 1911 to the end of Japanese war in 1945. Stilwell, the man, was, after all, just a bystander in the early years. *Stilwell*, the book, gets to the Sino-Japanese War (1937) by great leaps and bounds—27 of its 35 years in but 164 of its 531 textual pages. One must question whether the jump from Yuan Shih-K'ai in 1911 to Chiang Kai-shek in the 1940s, or from William Howard Taft to Franklin Delano Roosevelt, can be made with such dispatch. Given the book's concluding doubt about an American solution, it might be asked whether Chiang or Roosevelt or Stilwell, for all his idiosyncracies, were not acting out the last scenes of a drama foreshadowed in its less intense, earlier acts.

Even the hero's personal magnetism acts to

subvert rather than overcome the problems of generalization. Stilwell is so dynamic one almost forgets that there were other "attachés"—military and diplomatic, public and private, economic and religious—with other varieties of experience. Such characters pass through the broad, effective Tuchman panorama, but as passive participants in the Stilwell theater. They were, in reality, of course, the active agents of American material and culture. Given a historical and not biographical focus, Stilwell's experience is more a product of them than theirs is of him. The book's effort is a noble one, but it shows the need for more analysis of the component parts of American foreign policy. The shared assumptions and attitudes of all Americans in China gave United States policy its functional definition into which Stilwell's own role fit so well.

One critic concerned with the endless desert footage of a contemporary film classic commented he searched for more "Lawrence" and less "Arabia." The opposite is true here. We now have had sufficient Stilwell. We need to search about for more China, and indeed for more America if we are to go beyond biography to understand as well as describe the nature of "the American experience in China."

JERRY ISRAEL

Northern Illinois University

HAROLD SEYMOUR. *Baseball: The Golden Age*. New York: Oxford University Press. 1971. Pp. viii, 492. \$12.50.

The author of this book has a Cornell Ph.D., has taught college-level history for fifteen years, and displays many of the hallmarks of the serious academic scholar. He has put his book together in an orderly fashion (particularly in comparison with some sportswriters' books), and he discusses the sort of thing that is of interest to historians even if they happen not to be baseball buffs: organized baseball's commercial structure, its almost total authority over players, its cynical attitude toward a trusting public, and its reflection of the varied social and economic changes in the nation at large. Seymour handles all these themes skillfully, especially his analysis of the game's business operations, but he occasionally strains for

a pretentious academic posture that has the effect of making a mountain out of a pitcher's mound. He has directed this book to the general reader and has omitted footnotes; a bibliographical section at the end is generally helpful but leaves the reader uninformed as to the specific source for many of the author's most important statements.

Seymour is not only a scholar but a baseball nut: he was batboy for the Brooklyn Dodgers in the 1920s, played the game in high school and college, and has coached, umpired, and scouted. He was a youthful admirer, or at least observer, of the men he writes about here. Half memoir and half popular history, his book is vastly superior to the usual gee-whiz banalities of the daily sports page. Like most sports fans, Seymour is often nostalgic, especially about the goings-on at Ebbets Field, but he is never uncritical. He is that rare person, a batboy-turned-iconoclast, and his book is full of unsentimental revelations: baseball owners in the good old days were as greedy as they are now, Ty Cobb was psychotic, Babe Ruth was uncontrollably and childishly self-indulgent, the great 1927 Yankees achieved superiority by raiding the impecunious and approachable Red Sox for all their best players, the "Black Sox" of 1919 were indeed crooked but so were scores of other players who never got caught, and Judge Kenesaw Mountain Landis (the first baseball commissioner) was an arbitrary old martinet.

Seymour approaches his material with the raised eyebrow of the skeptic, not the open mouth of the credulous. "To call professional baseball a sport and the players and owners sportsmen," he writes, "is to use language so loosely as to divest it of meaning. Games staged for money are not played for fun." Baseball is really "a boy's game played by grown men for a living and run by promoters for a profit." But, he adds, it is after all "a cut above sticking barbs into bulls, or having two men batter each other in a ring until one drops unconscious."

Seymour has previously written *Baseball: The Early Years*, covering the late nineteenth century. A third volume on the recent history of the game is in preparation.

ROBERT L. BEISNER
American University

CHARLES LARSEN. *The Good Fight: The Life and Times of Ben B. Lindsey*. Chicago: Quadrangle Books. 1972. Pp. xi, 307. \$10.00.

Ben Lindsey (1869-1943) was a rather simple man in basic intellect and character who had a highly complex career, being a juvenile court judge in Denver, a participant in Progressive politics, a voyager on the Ford peace ship, an advocate during the 1920s of changes in the institution of marriage, and a California Superior Court judge in the 1930s. An individual of great renown in his lifetime, Lindsey became unknown to the public and neglected by scholarship within a generation after his death. Charles Larsen's study, an authorized, but "not an official biography," attempts to reclaim Lindsey from undeserved obscurity and generally accomplishes its task, especially, for the post-1914 aspects of his career.

The earlier portions of the book are disappointing. The chapters on Lindsey's youth and the establishment and operation of the juvenile court offer little information or interpretation not in Lincoln Steffens's *Upbuilders* (1909) and the judge's own autobiographies. Larsen does not sufficiently place Lindsey's juvenile court work within a context of national trends, nor does Larsen discuss the weaknesses of this institution, a topic that has received much recent attention. Considerable space is given, however, to Lindsey's reflections on the 1912 presidential candidates. Yet, as the author admits, being judge of the Denver Juvenile Court was Lindsey's "chief role in life."

The book improves markedly as it proceeds and is particularly insightful on Lindsey's activities in the 1920s as a marriage and sex reformer. Larsen exaggerates, perhaps, in describing the judge as the "Prophet of the Jazz Age," or a "spokesman for . . . 'Flaming Youth,'" the more so since Larsen has demonstrated the modernity of Lindsey's program. Here, as on the Ku Klux Klan skirmish, the 1929 disbarment, and the California comeback, there is more detail and analysis.

Larsen's laborious research is based on the immense, though inadequately organized, Lindsey manuscript collection in the Library of Congress. Larsen has also made good use of "the dwindling but helpful group" who knew the man. The overall perspective is favorable to Lindsey, but the biographer is aware of his

subject's limitations. The same perspective is applicable to the biography. It is a well-researched and well-written book that fills a gap in the bibliography of reform and will help restore Lindsey to importance, but it is a book flawed by its perfunctory treatment of the pre-World War I phase of his Denver career.

PETER GREGG SLATER
Dartmouth College

RICHARD B. HENDERSON. *Maury Maverick: A Political Biography*. Foreword by JOE B. FRANTZ. Austin: University of Texas Press. 1970. Pp. xxiii, 386. \$8.50.

"Two cowboys can ride herd a whole lot better than one," FDR once wrote to Maury Maverick in a sincere compliment to the aggressive, hard-driving little Texas congressman who had given the president's New Deal program a needed lift on more than one occasion. Maverick served only two terms in Congress, but he attracted more attention during his four years in the House of Representatives than many congressmen who have service records that span a score of years. But this was Maverick's style. As one of the leading members of the San Antonio Citizens League he helped oust a political machine described by one Texas politician as being capable of giving "Tammany Hall of New York lessons when it comes to holding crooked elections and counting candidates out who have been honestly elected." As an astonishingly liberal Southerner he organized a group of like-minded congressmen into a determined little voting bloc shortly after his arrival in Washington as a newly elected freshman congressman. After his defeat for re-election in 1938 Maverick won a term as mayor of San Antonio and did so much to modernize this beautiful but neglected city that John Gunther concluded that Maverick was "incontestably the best mayor San Antonio ever had." Even as head of one of the many wartime agencies, the Smaller War Plants Corporation, Maverick caught the nation's attention during World War II with his campaign against deadly bureaucratic jargon, coining the word "gobbledygook" to describe it.

Chronicling the colorful Maverick's political career has provided Henderson with a difficult challenge. Although asserting in his preface

that Maverick's family urged him to paint an objective picture of the outspoken Texan, warts and all, it is obvious that the biographer is much taken with his never-commonplace subject. Maverick's "volatile nature," as James A. Farley described it, had led Maverick to make some undeniably outrageous remarks, yet Henderson could almost always find excuses for the congressman's rough language, often employing such delicate phrases to describe it as "affectionate profanity" or "studied irreverence." But in truth Maverick was an attractive subject. An ardent New Dealer after President Roosevelt began to see that the days of his reform program were numbered, Maverick never weakened in his resolve and was willing to be a party maverick (his grandfather contributed the word to our lexicon) if necessary. Perhaps even more admirable was his devotion and effectiveness as a civil libertarian. Roger N. Baldwin, director of the American Civil Liberties Union, paid him the highest of tributes: "Among the handful of civil rights champions in Congress over the years, Maury stands out as the most devoted and the most skillful in achieving what he went after."

Henderson has written a compelling and well-documented monograph. He might have been more effective in relating Maverick with his times, thus achieving a smoother narrative, but the book is nevertheless a substantial contribution to the historiography of the Roosevelt-Truman era. An attractive volume with a section of photographs and a foreword written by Joe B. Frantz in his delightfully humorous style, Henderson's biography is also a fine reference book with a good bibliography and a functional index, always helpful to a historian in a hurry.

ROBERT W. LARSON
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RICHARD M. FREELAND. *The Truman Doctrine and the Origins of McCarthyism: Foreign Policy, Domestic Politics, and Internal Security, 1946-1948*. New York: Alfred A. Knopf. 1972. Pp. xii, 419, xii. \$10.00.

This book, originally a doctoral dissertation under the direction of Gabriel Kolko, is an interesting and somewhat distinctive New-Left interpretation of the genesis of McCarthyism.

A scholarly polemic of a nature currently fashionable, it will, I suppose, be praised and damned in accord with already established views on the school of thought it represents. But the book is sufficiently provocative to merit serious consideration on its own and should be read by anyone interested in the cold war and American civil liberties.

Professor Freeland's thesis is that after the war the Truman administration became convinced that massive economic aid to Western Europe was necessary to develop American economic and strategic interests (largely the former) on the Continent. In order to build sympathy in Congress and throughout the nation for the Marshall Plan, administration officials mounted an anti-Communist propaganda offensive that produced a rigidly anti-Russian foreign policy and inadvertently paved the way for Joe McCarthy. In one year, from March 1947 to March 1948, the attitude of the American people toward the Soviet Union shifted dramatically; the public debate on the Marshall Plan was the "main battle" in the forging of a cold-war outlook that plunged us into the hysteria of the early 1950s and continued with sufficient force to commit us to Vietnam. Singled out for special attention are the Federal Employee Loyalty Program, the attorney general's list, efforts to deport "subversive" aliens, administration support of the House Committee on Un-American Activities, the release of various diplomatic documents that appeared to reveal Soviet hostility and imperialism, and the dispatch of the Freedom Train—a device that attempted, according to Freeland, to elevate the Truman Doctrine to the level of the Constitution.

Numerous efforts have been made, of course, to pin the blame for the cold war and McCarthyism on Truman, and this study suffers from some of the defects of its predecessors. It underestimates the role of Congress in the drive for internal security legislation, minimizes Truman's contributions to the defense of civil liberties, exaggerates the political uses of anti-Communism prior to Dewey's defeat, and, above all, makes little effort to describe those events at home and abroad that might have given Americans good reason to fear Communist aggression and subversion. Still, Freeland raises numerous questions about the Marshall

Plan and its advocates that need answers. And the extent of the author's research is to be applauded: he combed the Truman Library, the Library of Congress, and other archives for relevant materials; interviewed Clark Clifford; and read widely in the literature of the period under examination. Much of the evidence needed to prove anything conclusively about the events covered in this book remains beyond the reach of scholars, but Professor Freeland is to be congratulated for posing a challenging thesis that should stimulate historians for a long while.

THOMAS C. REEVES
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GRAHAM T. ALLISON. *Essence of Decision: Explaining the Cuban Missile Crisis*. (Written under the auspices of the Faculty Seminar on Bureaucracy, Politics, and Policy of the Institute of Politics, John Fitzgerald Kennedy School of Government, Harvard University.) Boston: Little, Brown and Company. 1971. Pp. xii, 338. \$4.50.

LOUISE FITZSIMONS. *The Kennedy Doctrine*. New York: Random House. 1972. Pp. 275. \$7.95.

JAMES E. MCSHERRY. *Khrushchev and Kennedy in Retrospect*. Palo Alto, Calif.: Open-Door Press. 1971. Pp. 233. \$8.95.

Partisans of the tragically short Kennedy administration have often singled out the president's handling of the 1962 Cuban missile crisis as his finest hour in the field of foreign affairs. In the ten years following those thirteen days when the world's two most powerful nations "paused at the nuclear precipice," critics of the Kennedy mystique have raised serious questions about the necessity and wisdom of that policy. Only one of these books deals exclusively with the missile crisis, but all three, for quite different reasons, consider it the touchstone of Kennedy's foreign policy and a "major watershed in the Cold War."

Because of its analytical complexity Allison's book deserves a brief summary. Writing under the auspices of the Faculty Seminar on Bureaucracy, Politics, and Policy of the Institute of Politics in the John Fitzgerald Kennedy School of Government at Harvard, Allison describes the decision-making process during the Cuban missile crisis in the United States and the Soviet

Union by assigning three hypothetical analysts to interpret the event using three different conceptual models. First there is the classical or "rational actor" model, which is most often employed by diplomatic historians to explain foreign-policy decisions. It is based on calculating what rational, strategic choices would be made by key individuals for their nations without reference to any of the internal, personal, or bureaucratic machinations within their respective governments. Viewed from this conceptual frame of reference, the missile crisis, it is assumed, can best be explained in terms of value-maximizing choices like the following: the Soviets attempted to place defensive and offensive missiles in Cuba in order to obtain missile-power parity with the United States; the American government responded with a blockade as the logical middle course between doing nothing and invading Cuba; this in turn shifted the burden of further escalation back to the Soviets, who finally withdrew their missiles not because of the blockade itself, as most published accounts have claimed, but because it was coupled with the implicit threat of an air strike or land attack.

The second process model is one developed by organizational theorists and economists. It assumes that international politics is made up of organizational outputs that are invariably standardized and sluggish in times of crisis because they are based on routines (SOPs) for dealing with standard or normal day-to-day situations. From this vantage point, the actions of the United States and the Soviet Union do not have to be molded to fit a particular rationale. Therefore what started out to be either an attempt to bridge the missile gap or simply an attempt to defend Cuba (with the emplacement of defensive [SAMs] and offensive missiles [MRBMs]) was complicated and confounded by competition between the GRU, the KGB, the Air Defense Forces, and the Strategic Rocket Forces. Each agency did what it knew best to do without concern for internal consistency or adherence to the same security standards. Allison suggests it is possible that IRBMs, instead of only MRBMs, were installed by the Strategic Rocket Forces without Khrushchev's direct knowledge.

According to this second process model the United States chose to blockade Cuba out of

confusion arising over intelligence information because of competition between the CIA and the Air Force and because of a misunderstanding between civilian and military leaders over the effectiveness of a "surgical" versus a "massive" air strike against Cuba. Finally the Soviets withdrew their missiles, partially because of the confusion created by the few top Soviet officials who actually were aware of their emplacement and partially because actions by competing agencies created a contradictory situation in which the left hand of the USSR did not know what the right one was doing. So just as "Kennedy was beginning to wobble, Khrushchev folded" because of internal problems of communication and coordination.

The third process model describes the bargaining games played by top policy makers based on their unequal positions of power within governmental circles. According to this pluralist model foreign-policy decisions are the product of decentralized coordination (shared power) of the various pressures emanating from representatives of interest groups inside and outside of government. Using this conceptual approach Allison speculates that the Soviet Union placed its missiles in Cuba as the result of pulling and hauling among select members of the Presidium about which we do not as yet have enough public information for documentation. But there is already an abundance of this type of evidence for the United States in the published personal accounts of the meetings of the ExCom (Executive Committee of the National Security Council), which Kennedy created to deal with the crisis.

This third model questions whether the Soviet missiles in Cuba actually affected the nuclear balance of power and instead concentrates on the idea that, because Cuba was already a "political Achilles' heel" as a result of the Bay of Pigs fiasco, practically all of the influential forces represented in the ExCom pushed Kennedy in the direction of strong action. From the beginning, therefore, there was little chance of negotiating through straight diplomatic channels, especially in view of the argument that such normal diplomacy would allow time for the missiles to become operational. Thus, the blockade was really a "collage" decision, arising less out of organizational sluggishness and confusion and more out of in-

tense bargaining and pressure tactics within the ExCom. Likewise, the Soviets possibly decided to withdraw not because of the blockade or because of the subsequent ultimatum, but because of a tacit sense of partnership and mutual consideration between Kennedy and Khrushchev, which led them to establish direct lines of communication to avoid misunderstanding each other's intentions. This ultimately led to a private deal between them, which Robert Kennedy personally conveyed to Soviet Ambassador Anatoly Dobrynin (after delivering the official ultimatum): namely that American missiles in Turkey and Italy would be removed "within a short time after the crisis was over."

There is no doubt that Allison's multiconceptual approach adds new insights and speculative dimensions to our understanding of the complex process of decision making during foreign-policy crises which will prove invaluable for analyzing present and future ones. This study asks that diplomatic historians stop assuming that rational goals account for a nation's choice of action and begin to concentrate on the intragovernmental factors that determine any given foreign-policy outcome. However, it should be noted that the second and third models require information not readily, if ever, available about recent developments in foreign affairs, let alone past ones. Also, as far as the Cuban missile crisis is concerned, none of Allison's hypothetical analysts address themselves to the nagging question of the role that the forthcoming congressional elections may or may not have played either within the ExCom itself or among the much smaller group of the president's personal advisers. None of the process models, with the possible exception of the third one, seems to be designed to account for the ubiquitous ways in which "important interest groups" (technically outside of government, like various segments of the business community) influence diplomatic decisions.

Allison would have us believe that the formulation of foreign policy, at least in time of extreme crisis, is conducted by small *ad hoc* groups in both the Soviet Union and the United States, which attempt to cut themselves off from governmental bureaucracy and other influential pressure groups. Yet his evidence in the case studies for the second and third process models suggests how impossible this at-

tempt proves in practice, since "members of any organization, particularly career officials, come to believe that the health of their organization is vital to the national interest . . . [and] the President and senior players will almost always be concerned about domestic implications." There is no confusion, however, about the different lessons drawn from the three process models. While the first one leads to the conclusion that nuclear crises are manageable, the second and third ones clearly demonstrate how easy it would be for nations to stumble into a nuclear exchange because of the heretofore little understood risks in the decision-making process during diplomatic crises.

To Louise FitzSimons all the foreign-policy actions of the Kennedy administration add up not only to a defense of the international status quo, but also to an intensification of the cold war through the introduction of counterinsurgency tactics and nuclear brinkmanship. With particular reference to the emplacement of Soviet missiles in Cuba FitzSimons concludes that the crisis was as dangerous as it was unnecessary. These same charges were made by I. F. Stone and Ronald Steel several years ago and are based on the argument that Kennedy arbitrarily raised the incident to a high-level crisis because of his excessive concern for his own and his country's prestige (in the wake of the Bay of Pigs and his poor showing at Vienna with Khrushchev) and because of the November mid-term elections. Thus he bypassed traditional diplomatic contacts in favor of "eye-ball to eye-ball" confrontation. Like Stone and Steel, FitzSimons reaches this conclusion through logical deduction (Allison's model 1) rather than through documentary proof or detailed analysis of the decision-making process.

Generally speaking, FitzSimons too narrowly ascribes to the Kennedy administration alone an "untenable globalism" that led to "increased areas of conflict, to a heightening of the arms race, and to American concern with and involvement . . . in the affairs of almost every country of the world." This is in essence the Kennedy Doctrine. It is based on the assumption that since "we can . . . we should affect the course of events around the globe." While Kennedy indeed was a cold warrior it is questionable whether his counterrevolutionary

foreign policies were qualitatively (rather than simply quantitatively) different from all such presidential warriors since 1945. Richard J. Walton's book, *Cold War and Counterrevolution* (1972), is more balanced and better documented on this point.

Aside from exaggerations about how Kennedy exacerbated the cold war single-handedly, FitzSimons, a former foreign affairs officer for the Atomic Energy Commission and staff assistant in the Senate, generally makes judicious use of all the published secondary accounts of the Kennedy years plus congressional documents, the oral history collection at the JFK Library, and the one-volume Bantam edition of the Pentagon Papers. For obvious reasons this work does not accord equal attention to the more positive aspects of Kennedy's policies in Africa and Latin America. For other areas of the world, however, it provides a needed counterweight to the over-apologetic accounts of Kennedy's foreign policy by Sorensen, Schlesinger, Hilsman, and others, which have been the standard fare to date. She also rightly laments the fact that Kennedy used his charisma to stimulate cold war attitudes among Americans rather than to eradicate them.

In contrast to the works of Allison and FitzSimons, the McSherry book is an intellectual disaster. It is both the author's and publisher's intent to present "a serious, conservatively oriented book," despite the difficulties inherent in doing so because of the "growth of lock-step liberalism in the past thirty years." Unfortunately they have not succeeded. Based on inadequate research (primarily newspaper accounts and a handful of secondary sources) this book does not attempt the systematic analysis of the diplomatic relationship between Kennedy and Khrushchev that the title implies. Instead, the president surfaces only sporadically in what is really a superficial summary of Soviet policy from 1957 to 1964.

A former State Department intelligence specialist during the Eisenhower administration, McSherry drags out all of the cold war clichés from the domino theory to the fact that all Communist nations are equally untrustworthy and only understand force. Hence, approving of Kennedy's forceful handling of the Cuban missile crisis, McSherry views it simplistically as a personal attempt by Khrushchev "to

expose Uncle Sam as toothless and feeble" under the leadership of a young "tissue-paper president." The writing is disjointed and rambling, which makes the vague and poorly documented charges against Khrushchev and "mush-minded" American liberals all the less convincing. Contrary to his intent McSherry has not strengthened the scholarly reputation of the Far Right with this book.

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LYNDON BAINES JOHNSON. *The Vantage Point: Perspectives of the Presidency, 1963-1969*. New York: Holt, Rinehart and Winston. 1971. Pp. x, 636. \$15.00.

LADY BIRD JOHNSON. *A White House Diary*. New York: Holt, Rinehart and Winston. 1970. Pp. ix, 806. \$10.95.

Historians will long quarrel about the extent to which ghosts intrude between the authors of these two books and the published works, but such quibbling will be beside the point. Each book says what its author wants to have said, and each, beyond question, has the flavor, both linguistic and moral, of its author. The nearly simultaneous publication so soon after the event reveals a joint urgent need for apologia, but neither does this diminish the significance of the two books. Indeed the manner in which the apologiae are presented enhances the significance of these court reminiscences.

Mrs. Johnson (one might say "Ms." in deference to the South, but that would be mistaken as a nod to Women's Lib, whose principles seem to be pretty well rejected by Lady Bird Johnson's whole approach to her husband and family) recounts that on their arrival at the White House on December 7, 1963, "Mr. West was waiting at the door for us. . . . He has worked at the White House for twenty-three years and is now Head Chamberlain." Conversely, for January 20, 1969, Lyndon Johnson reflects: "the last morning I would wake up in this room, the last day I would live in this house. It was the day the mantle of the American Presidency would be shifted, in one of the greatest pageants of our democracy, from my shoulders to those of Richard Nixon. . . . Even in those final hours there could be no departure

from the routine. Presidents come and go, but the Presidency goes on without interruption."

Despite the intense interest in the politics of the Johnson regime, I suspect that the long-range historical interest of these two documents will lie far more in what they reveal about the nature of the American presidency than in anything they can tell us about the politics of the Great Society or the "war" in Vietnam. President Johnson refers to the "pageants" of the American democracy, but what he really seems to be saying is: "The king is dead; long live the king." The principal qualification, perhaps, is that being still alive, he can take several sharp pokes at the succeeding monarch for allegedly undercutting Johnson's Vietnam diplomacy during the extended lame-duck period.

We read in these records the account not just of a democratic presidency (in all its facets) but of an *imperial* democracy. It is impossible not to recall the strictures of Woodrow Wilson's favorite constitutional commentator, Walter Bagehot, who argued the desirability of separating the "dignified" from the "efficient" elements in a constitution. Wilson, for all his basic conservatism, never quite grasped this point, although he did see the point about the need to create "responsible government" by offering to resign upon defeat—and, perhaps not surprisingly, it was his second Southern successor, Johnson, who put the precept into practice. And Johnson put it into practice principally because Wilson, F. D. Roosevelt, and Harry Truman—his prime mentors in the presidential craft—had presided over the evolution of America from an expansionist to a defensive empire. Johnson, in a very real sense, suffered the consequences of presiding over a sated empire—an empire that had never suffered a serious military defeat (bar the controversial outcome of 1812–14), an empire that had enforced its sphere of influence in the Americas, made the Pacific "an American lake," and accepted from declining European empires the responsibility of keeping secure a world-wide market society. Defeat in these circumstances left no alternative to abdication.

For the real history of the Vietnam War one has to go to other sources. But for the impact of the war (and of crises in the Caribbean, the Middle East, and Central Europe) upon the

presidency these two books will remain primary sources. Mrs. Johnson's book, especially, records the interlocking burdens imposed upon the president by virtue of his being at once head of state and chief executive. Her account of the White House as court, despite her own remarkable sharing of the burdens and opportunities, leaves no doubt about the immense extra drain on one man's energies (and competence?) when he has to be both king and prime minister. And the "dignified" as opposed to the "efficient" aspect of his office was intensely absorbing for Lyndon Johnson.

A world-shocking tragedy shot Johnson into an office for which, in ordinary circumstances, he would have been as well prepared as had been most of his predecessors. But the circumstances cast him in the role of "populist" usurper. A prince had been murdered, and the horrified chorus of criticism seems, in retrospect, to have been more paranoiac than any paranoia as yet ascribed to American populists. It is little wonder that both of the Johnson books devote much space to their relationship with the Kennedys—which both books depict as one of warm mutual regard. Nor is it surprising that Lyndon Johnson (like the first Roosevelt) swore to carry out his assassinated predecessor's policies intact. In the domestic field, as Johnson records with loving attention, he accomplished this—with more success than would likely have been the case had the event in Dallas never occurred (although Johnson does not put it quite this way). In the field of foreign affairs, Johnson is even more careful to point to the unbroken thread of policy: "I would devote every hour of every day during the remainder of John Kennedy's unfulfilled term to achieving the goals he had set. That meant seeing things through in Vietnam as well as coping with the many other international and domestic problems he had faced. I made this promise not out of blind loyalty but because I was convinced that the broad lines of his policy, in Southeast Asia and elsewhere, had been right. They were consistent with the goals the United States had been trying to accomplish in the world since 1945."

The circumstances of the succession account for much, as does the nature of the office itself. Yet the background of the Johnsons themselves and the felt need to "justify" the South also

bulk large. Lyndon Johnson was deeply concerned with this latter problem (as was his wife, whose favorite politician seems to be Harry Truman). LBJ puts it this way: "The burden of national unity rests heaviest on one man, the President. . . . One reason the country could not rally behind a Southern President, I was convinced, was that the metropolitan press of the Eastern seaboard would never permit it. My experience in office had confirmed this reaction. I was not thinking just of the derisive articles about my style, my clothes, my manner, my accent, and my family—although I admit I received enough of that kind of treatment in my first few months as President to last a lifetime. I was also thinking of a more deep-seated and far-reaching attitude—a disdain for the South that seems to be woven into the fabric of Northern experience."

LBJ certainly brought with him to the White House much that shocked the Eastern establishment. But he also brought a Southern political know-how that in sheer managerial skill should have put that establishment to shame. What the cold-war liberal sophisticates of the East seemed not to recognize, and what is made very clear in *The Vantage Point*, is that the Southern populist had bought their own goals. What he brought to the job was a power of simplification that the Easterners found distasteful and even vulgar. In following through the logic of their own policies the usurping president revealed the limits of power and the need to call upon an essentially illiberal patriotism to justify contradictions between foreign and domestic actions. "Those who created division," writes Johnson, "who opposed decisions, and who made it more difficult to accomplish the job need to reflect on the consequences of their actions." Ultimately he refuted the doubters by very simple questions: "I wonder what the reaction would have been in the America of 1969 if Soviet cosmonauts had planted their red flag on the moon."

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HERBERT L. MATTHEWS. *A World in Revolution: A Newspaperman's Memoir*. New York: Charles Scribner's Sons. 1971. Pp. 462. \$12.50.

Herbert Matthews was one of those fascinating

men who made history while writing it. He repeatedly took positions and reported events that engendered strong reactions by governments and the public. Accused of being pro-fascist for his reports on the Italo-Ethiopian War, attacked by conservatives and Catholics for his reports of Loyalist activities during the Spanish Civil War, and widely condemned as having helped hoodwink the American government into believing Castro was not a Communist, Matthews has written an impressionistic memoir of his own career as a reporter for the *New York Times*—a career that clearly places him within the mainstream of American liberalism.

The book consists of a series of interesting and well-written chapters, each dealing with a country he covered as a reporter. Though he bases his comments largely on his own experiences he does not limit himself to just the time he spent there. Rather, he perceptively uses his past experiences as a means of commenting broadly on each nation and the world at large. His recollection of the ignorance of Hindu and Muslim peasants about the activities of Gandhi and the Muslim League during World War II becomes an opportunity to comment on nationalism as a middle-class emotion and concept. Accusations of bias in his reporting on the Spanish Civil War and the Italo-Ethiopian War bring on a discussion of how a good reporter must combine bias and factual honesty. A brief and relatively quiet tour in Italy from 1939 until 1941 provides the backdrop to an incisive and enlightening interpretation of Italian politics from Mussolini to Togliatti. Through all of this a number of themes are dominant. Matthews is clearly disillusioned with ideology. Communism (he sees all post-Lenin Communists as counterrevolutionaries), fascism, capitalism, socialism—they all seem irrelevant in the face of rampant nationalism. (Spaniards fought each other with little thought given to ideology.) His archenemy is authoritarianism, regardless of the disguise. And his faith is in the young radicals who may not be right but must create their own world in which to live.

The book also provides only a limited amount of useful primary source material, for Matthews has written nine other books in which most of his reminiscences of the three great crises he covered—Abyssinia, the Spanish

Civil War, and Castro—are fully recorded. He does relate the details of a fascinating interview between Ernest Bevin and Arthur Sulzberger on the subject of Truman's recognition of Israel, and there are also occasional quotes and short stories that catch the historian's eye; for example, when Matthews quotes (by hearsay) Robert Murphy in 1944 as saying that communism in Italy was no problem since the Communists "are now part of the democratic community" (p. 137). The real scholarly value of this book, however, is for historians of American journalism. Matthews states at the beginning that he is happy to report at last on his two great struggles (the Spanish Civil War and Castro) with "his" newspaper with complete candor. He defends his "honest bias" and accuses the paper of failing to live up to its own credo when it suppressed his reports on Castro. Yet this is clearly criticism from a member of the family. Equally intriguing is the insight this provides into the relationship between the reporter, who sees only part of the whole but knows what he sees, and the publisher-editor who looks at the whole but sees nothing for himself. During the Spanish Civil War the *New York Times* incensed Matthews when the editors chose to give equal weight to reports on the Loyalists from Matthews at the front and to reports on the Rebels based on official press releases. Then during the early years of Castro in Cuba the paper took the other position and "muzzled" Matthews, in spite of (or because of?) his exclusive access to Castro and other Cuban government leaders. There are other curious revelations, particularly Matthews's offer in 1962 to Ambassador Thomas Mann to report back any items of interest that came up during his projected talks with the Cuban leaders; that is, he would check back with the CIA!

There is no doubt that much of this book is an attempt at self-justification by Matthews, particularly regarding his relationship to Castro, and on that subject Matthews's arguments are persuasive. The popular notion of Matthews single-handedly influencing the State Department's image of Castro is asinine on the face of it. The book is also an earnest plea for the value and validity of newspaper reporting. He quotes Arthur Schlesinger, Jr.'s comment before the American Historical Association to the effect that, after having served in the White

House, he would no longer take newspaper and magazine articles seriously. Matthews archly points out that if one "cannot distinguish good journalism from bad, or trustworthy newspapermen from phonies, he should not try to write contemporary history" (p. 8). But for Herbert Matthews separating the honest from the dishonest in the stories he covered is simple, for he knows what he saw. For historians it is not so easy, for like publishers and editors, we must look at all sides and always through the eyes of others.

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ROBIN W. WINKS. *The Blacks in Canada: A History*. New Haven: Yale University Press. 1971. Pp. xvii, 546. \$15.00.

This is the first comprehensive history of Canadian Negroes—from the arrival of the first African slave in New France in 1628 to the scattered stirrings of black protest activity in the late 1960s. It is an extraordinary achievement, impressive as much for its wide-ranging breadth as for its scholarly depth and thoughtful insights. It is far more than black history, moreover, for Professor Winks deals importantly with Canadian, Canadian-American, and French and British imperial history as well. The black presence in Canada has never been as significant as in the United States, from which many Negroes originally came, for blacks have never amounted to more than five per cent of the population of any province and only about two per cent of the nation as a whole. Scattered from the Maritimes to Vancouver, the product of several distinct waves of immigration widely separated in time and area, for the most part poorly educated and overwhelmingly lower-class throughout their history, isolated both from the dominant bicultural white society and from each other, Canadian Negroes have had even less success than their black brothers in the United States in gaining an acceptable and accepted place in Canadian life. Too few in number to have more than local visibility, too scattered to have an effective press or national leaders and organizations, the blacks have largely been ignored by white Canadians, who smugly and er-

roneously assumed that racial problems and prejudice existed only south of their border. In Winks, Canada's blacks have found a sympathetic and indefatigable chronicler, who over more than a decade mastered an enormous range of chiefly unpublished sources scattered throughout Canada and the United States as well as in the United Kingdom, France, the West Indies, and even West Africa. Since only about half of his research materials were incorporated into this book, Winks has deposited his notes, correspondence, and other papers, including early and more extensively documented drafts of the manuscript, in the Schomburg Collection of the New York Public Library for use by other scholars. Although he modestly points out that because of its pioneering character this cannot be regarded as a definitive study, it is unlikely to be replaced soon. Indeed, it may become a Canadian equivalent to Gunnar Myrdal's epochal *An American Dilemma*, by calling attention to the long and as yet unrealized struggle of Canadian Negroes to become Negro Canadians.

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FREDRICK B. PIKE. *Hispanismo, 1898-1936: Spanish Conservatives and Liberals and Their Relations with Spanish America*. (International Studies of the Committee on International Relations, University of Notre Dame.) Notre Dame: University of Notre Dame Press. 1971. Pp. xx, 486. \$15.00.

The year 1898 brought to an end Spain's overseas empire and stimulated Spanish intellectuals and statesmen to probe the causes of national decadence and to reappraise their country's place in world affairs. This year of disaster and despair for Spain has been chosen by Fredrick B. Pike as the point of departure for an examination of the theory and practice of Pan-Hispanism. In scope this work is much broader than would appear from the narrow definition implied by the title. Not only does the author deal with material going back to the 1870s, but he explores in considerable detail intellectual, social, political, and religious problems of Spain during the past century.

Focusing upon the bitter debate between conservative and liberal Pan-Hispanists, the au-

thor depicts an international movement that was more significant for rhetoric than for practical achievements. Liberals and conservatives, while differing on religious and social issues, agreed on the need to preserve a hierarchically and organically structured society. Democracy and materialism, in their view of the world, were the archenemies of the Hispanic *raza*. If Latin Americans wished to defend themselves from the twin threats of Yankee imperialism and social revolution, in the opinion of *hispanistas*, they were urged to cultivate the traditional values of Spanish spirituality.

Although the ranks of Pan-Hispanic advocacy included such prestigious figures as Rafael Altamira, Ramón Mendéndez Pidal, Miguel Unamuno, Antonio Maura, and Ramiro de Maeztu, the movement never enjoyed more than limited success. From time to time important Spanish Americans like Rubén Darío, José Enrique Rodó, Marcelo T. Alvear, and Manuel Gálvez showed interest in *hispanismo*, but the establishment of close political, economic, and cultural ties between Spain and her former colonies eluded the advocates of Hispanic cordiality.

Pike puts his finger on the basic shortcomings of Spanish leadership. An abrasive paternalism toward America alienated potential friends of Spain. Unseemly quarrels among conservatives and liberals reduced effectiveness. Lack of a sustained governmental commitment and Spanish weakness as a world power handicapped *hispanoamericanistas* in their efforts to compete with the United States and European powers in Latin America. The gradual drift of *hispanista* ideology to the political right limited the movement's appeal almost entirely to the most conservative elements in the New World.

The documentation offered by Professor Pike is extensive, varied, and convincing. Historical interpretations are perceptive and judicious throughout. An attempt to show connections between twentieth-century *hispanismo* and the Pan-Hispanic movement before 1870 would have given greater historical depth to this study, but this is a minor criticism that subtracts little from the overall excellence of Mr. Pike's scholarship.

MARK J. VAN AKEN
California State University,
Hayward

DAVID GREEN. *The Containment of Latin America: A History of the Myths and Realities of the Good Neighbor Policy*. Chicago: Quadrangle Books. 1971. Pp. xiii, 370. \$10.00.

This is a troubling book, though not entirely for the reasons the author intended. Part of the difficulty may be that David Green attempted to analyze several somewhat different, if related topics and did not altogether succeed with any of them. The preface, for instance, promises a revisionist analysis of "the myths and realities of the Good Neighbor Policy," but it is regrettable that the author tells us little new about this promising topic.

Green also discusses recent United States foreign economic policy and economic relations with Latin America. There is worthwhile material in chapter three on the Inter-American and Export-Import banks, chapter 4 (perhaps the best in the book) on the economic impact of the war, and parts of chapters 7 and 8. Elsewhere the author fails to provide a systematic economic analysis, despite his underlying and unproven assumptions about United States economic motives.

Green's arresting major contention is that the United States feared and persistently attempted to thwart Latin American nationalistic movements. He holds the New Deal's "relationship to private interests" responsible for the ultimate "tragedy," though at one point he concedes that the intentions of Presidents Roosevelt and Truman were benevolent. Otherwise Green implies insistently that even the motives were wrong. Thus he asserts that the United States consciously "retained effective control" over Cuba after 1933 and that Secretary Hull was primarily interested in securing markets.

Here Green largely ignores the effect of the depression on Washington's policies and misrepresents Hull's ultimate goals. Later he reads economic expansionism into Roosevelt's remark that "Dr. Win-the-War" had to replace "Dr. New Deal." Again Green claims that the United States subsequently neglected Latin America because of a shift "by mid-1944" of "economic resources into Western Europe for anti-Russian activities." Truman accordingly appears as a man of views "crude in the extreme" who preferred "confrontation," while Bernard Baruch (whose influence Green exaggerates), Nelson Rockefeller, and Arthur Vandenberg figure variously among the cast of villains. With even scantier evidence Green disposes of President Kennedy as a conscious heir to an antinationalist policy and an advocate of "containing" Latin America through the Alliance for Progress.

Insufficient research partly explains some of Green's errors, as in his misleading account of wartime Argentina. But the book also suffers from indiscriminating idealization of nationalism and from an unfortunate attempt to prove too much. Green, however, has properly identified an important opportunity for historical analysis and criticism of policy. This challenge remains for scholars.

PAUL S. HOLBO

University of Oregon

BRIAN R. HAMNETT. *Politics and Trade in Southern Mexico, 1750-1821*. (Cambridge Latin American Studies, Number 12.) New York: Cambridge University Press. 1971. Pp. viii, 214. \$12.50.

This important study concerns the attempt to control the lucrative cochineal trade of Oaxaca. It not only provides considerable information about the economic and political aspects of the eighteenth-century Bourbon reform program and its implementation but also gives a thorough discussion of the dye trade, which was, after bullion, the most important product exported from New Spain in the colonial era.

The red-dye cochineal was controlled by merchants in Mexico City, who, working closely with firms in Spain and through the local *alcaldes mayores*, exploited Indian labor, financed the cultivation and manufacture of the dye, and marketed the product in Europe. Cochineal brought such handsome profits to the merchants and production boomed to such an extent that the metropolitan government, ever desirous of more revenue, attempted to smash their monopoly and to tax the production by implementing administrative reforms and free trade. Hamnett explores in detail the many facets of the struggle to control the cochineal industry: the reform-minded administrators versus the traditionalist bureaucrats; the Mexico City merchants versus the Veracruz

Consulado; the Creole interests of the independence era versus the peninsulars; and the Church hierarchy in Oaxaca, which first supported the reform government and the intendancy system, thinking the Indians would be less exploited, and later threw its support to those elements within the government that wished to see the trade continued under the control of the Mexico City merchants.

All the while Oaxacan cochineal production was falling drastically, from a high of one and one-half million pounds in 1774 to 300,000 pounds in 1821. The drop was caused in part by government action, the insecurity of commerce between Spain and her possessions during the colonial wars, the chaotic economic conditions resulting from the struggle for independence, and ultimately the competition presented by other areas into which cochineal cultivation was introduced.

Hamnett has based his study largely on archival materials in Spain and Mexico. Lacking only in some analysis of the grass-roots implications, particularly within the Indian society, of this politico-economic struggle the work is otherwise thorough and excellent.

CHARLES R. BERRY
Wright State University

MOISÉS GONZÁLEZ NAVARRO. *Raza y tierra: La guerra de castas y el henequén*. (Centro de Estudios Históricos, New Series, Number 10.) [México, D. F.:] Colegio de México. 1970. Pp. x, 392.

Previous works by this outstanding Mexican historian have been noted for methodical and objective use of primary sources and first-hand statistical data, for example, *Estadísticas sociales del Porfiriato* (1956), *El Porfiriato. La vida social* (Volume 4 of *Historia moderna de México* [1959]), or *La colonización en México* (1960). One can now add another title to this list of basic reference works for modern Mexican history. *Raza y tierra* is based on archival searches in Mexico City, Merida, La Habana, Madrid, in the Foreign Office in London, and on an impressive number of printed reports, contemporary accounts, and secondary works. In the author's usual style—a kind of military ordering of all relevant information—the text is tightly packed with factual footnoted information and then reinforced with statistical ta-

bles and a rear-guard appendix of supporting documents, all serving to give an impression of overwhelming evidence.

Raza y tierra is actually four interrelated studies in Yucatecan history. The first one traces the unfolding of the tragic Caste War that burst into flames in 1848 and finally sputtered out in the early twentieth century. This part of the book serves as an important supplement to Nelson Reed's *The Caste War of Yucatan* (1964). The second study represents rare archival research on the Yucatecan slave traffic to Cuba, 1848–61. British abolitionist policy inhibited this business, but, according to González Navarro, it was the triumph of Juárez and, more important, the rise of labor-hungry henequen plantations that actually put an end to it. The third part is concerned with labor conditions and labor legislation in Yucatan. Here the author demonstrates that in spite of reforms that modified Indian peonage obligations, Mexican *gachupines* continued the Spanish colonial tradition of hacienda managers acting with police powers to pursue runaways, vagrants, and debtors and to administer corporal punishment. An old Spanish proverb still described the system: "Los indios no oyen sino por las nalgas." In essence there is agreement with the account of Indian servitude given by John K. Turner in his sensational exposé *Barbarous Mexico* (1911). The final section provides a statistically detailed account of henequen production and agrarian reform in Yucatan from the revolution of 1910 to the present, with emphasis on monoculture, collectivized *ejidos*, and peasant proprietorship.

Documentary attachments, a full bibliography, and an index by name and subject add the finishing touches to this scholarly production. Any shortcomings in a work by González Navarro must be taken with its virtues. The writer lets the facts, wet or dry, speak for themselves and only rarely splices in his own interpretive opinions, impressions, or comments.

ARTHUR F. CORWIN
University of Connecticut

RALPH DELLA CAVA. *Miracle at Joazeiro*. (Institute of Latin American Studies, Columbia University.) New York: Columbia University Press. 1970. Pp. xiv, 324. \$9.95.

There is no greater test of the historian's craft than the need to sift through conflicting and highly partisan secondary sources. To be the first investigator who also consults unpublished documents is an even greater opportunity. Ralph della Cava has met the challenge admirably in this painstakingly researched monograph.

Miracle at Joazeiro reconstructs the story of an extraordinary popular religious movement in an interior town of the impoverished north-eastern Brazilian state of Ceará between 1889 and 1934. The central figure was Padre Cícero, a devout and remarkably long-lived priest, twice excommunicated for failing to obey the ecclesiastical authorities who had declared fraudulent the "miracles" alleged to have occurred at the humble cleric's mass. Despite condemnation by the "Romanizing" Church, Padre Cícero rapidly became a saint to the thousands of destitute pilgrims who flocked to Joazeiro. In time the venerated priest became the most powerful political figure in the northeast and a fierce promoter of his adopted city of Joazeiro. Not surprisingly, he acquired many enemies, both among the clergy and outside observers who insisted on seeing only "fanaticism" in a movement whose deep social roots della Cava mentions briefly. The author has purposely avoided emphasizing the colorful folkloric aspects, despite obvious sympathy for the underprivileged masses for whom the movement meant most. Instead he concentrates on the difficult task of reconstructing the complex political history of Padre Cícero's relations with the "Romanizing" clergy of the contemporary Brazilian church, the state and national political elites, and José Marrocos and Floro Bartholomeu, the ambitious laymen who played such a key role in building the legendary priest's secular power.

The story is set clearly in the context of regional and national change, both political and economic. The author convincingly demonstrates how the "dualistic" hypothesis of a "civilized coast" and a "primitive interior" is refuted by the case of Joazeiro, which showed close connections with a regional economy and a national political system. Equally important, *Miracle at Joazeiro* offers fascinating insights into the structure and use of power in a community of the Brazilian northeast dur-

ing the Old Republic (1889-1930). One finishes this book with the conviction that it is the most objective and carefully documented political history of the movement that could now be written. Its analytical clarity and restrained style were made possible by the consultation of unpublished documents and oral testimony available only to a patient researcher willing to settle down in an interior city and listen.

THOMAS E. SKIDMORE
University of Wisconsin,
Madison

ADLAI F. ARNOLD. *Foundations of an Agricultural Policy in Paraguay*. (Praeger Special Studies in International Economics and Development.) New York: Praeger Publishers. 1971. Pp. xx, 294. \$17.50.

An attempt to establish bases upon which an agricultural policy may be developed for Paraguay is certain to encounter monumental difficulties. To explain these obstacles, the author provides a short geographical description of the country, a brief summary of political history, and a disappointing history of land tenure. These three introductory chapters fail to provide the background needed for understanding contemporary conditions. If the reader will turn first to Joseph Pincus, *The Economy of Paraguay*, published in this series in 1968, the gap can be filled, and Arnold's study will serve as an excellent elaboration of agricultural problems touched upon briefly by Pincus.

Justification for the book is found in the last four chapters. An examination of the minifundia-latifundia problem and the work of the Institute of Rural Welfare (IBR) suggests that there is no immediate prospect that the IBR can fulfill its mission. A chapter on "Paraguayan Plans for Economic Development and their Implementation" describes the objectives of three plans launched in 1965, 1967, and 1969. The last of these plans could result in great benefits if accompanied by what would be miraculous changes in Paraguayan bureaucracy. The problems have been identified, the solutions have been plotted; but implementation has failed miserably, except in a few cases such as road building.

The heart of the volume is chapter 6, "An

Approach to Agricultural Development." Here the author correctly endorses the agricultural infrastructure approach that emphasizes transportation, storage, many government services, and extensive contributions from the private sector. The concluding chapter attributes Paraguay's backwardness primarily to its "turbulent history" in which there was almost no road building and property rights were concentrated in very few hands. Most readers with a firm grasp of Paraguayan history will agree with the conclusion that "a strong central government may be the only alternative to anarchy and chaos" (p. 160).

One should emphasize that this is not a history of Paraguayan agriculture but an elaboration of what Paraguay must do in order to develop its agricultural resources. The author, who served in Paraguay with USAID from 1965 to 1970, has a thorough understanding of problems faced by agrarian reformers. His analysis of Paraguay's principal agrarian laws and the plan for 1969-73 show that Paraguayans know what to do; but it also raises the question of whether they will do it. The author is understandably pessimistic.

HARRIS GAYLORD WARREN
Santa Fe, New Mexico

Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

In its two central points Henry F. May's review of my *An Unsettled People* (*AHR*, 77 [1972]: 578–81) rather oversteps reasonable bounds. He appears to wish that I had written a different category of book, and—much worse because less obvious—he complains that the one I did write fails to prove a thesis that is, however, no part of it.

The book is frankly a synthetic effort; why should it not include “textbook topics” or cite “standard secondary works”? (It draws on primary sources, too, and could have cited more of them, but Professor May misreads my introductory remarks on sources and citations.) The critical question here surely is not whether the data, or even parts of the argument, are al-

ready familiar—indeed, the more familiar the better, if the sense made of them is fresh and valid. If a reviewer claims to find little more in a synthesis of social history than “the old miscellany,” he might at least be expected to give a few examples of its failure to integrate conventional topics (Professor May merely names a few of the latter). But I may misconstrue his point. Elsewhere in the review he quite effectively summarizes, with overall approval, the synthesis of leading topics, both novel and familiar. Perhaps he would have preferred a monograph, such as he urges me to write, or else a historiographical disquisition on other people's theories, models, and controversies, though he does not specify what “new ideas, information, and events” one should consider. But there is more than one way to ask “one's own questions” of history, and I hope the book will suggest some to monograph writers and followers of trends.

Professor May's other stumbling block is his recollection, flattering but unfortunate, of a conservative social hypothesis that I proposed in 1960 (*AHR*, 65 [1959–60]: 495–514). As he observes (quoting that article at much greater length than the book), it included a three-phase cycle running from colonial stability through nineteenth-century instability, anxiety, and reaction and back once again to relative stability in the twentieth century. The article may have been an “arresting skeleton,” but the book written in the intervening eleven years does not, I think, represent arrested development; it explicitly reverses the last part of the 1960 scheme. The first two parts, fleshed out, remain generally acceptable to Professor May. But when he comes to the last phase, he complains not only that the proof which he so long

though skeptically awaited is missing, but that the book actually proves "again and again" that, just as he suspected, American society has *not* perceptibly regained stability. (He also chides me for having failed to re-examine my hypothesis!) But the book is not the article, and sufficient to each are the errors thereof.

The most casual reader might well discount, on Professor May's own evidence, the first of his objections. But as it would take a reading of the book itself to reveal that his second line of criticism misrepresents its revised but still radically conservative argument, I trust that the review will dissuade no one from examining it.

ROWLAND BERTHOFF
Washington University

PROFESSOR MAY REPLIES:

I hope the same.

HENRY F. MAY
*University of California,
Berkeley*

TO THE EDITOR:

I hope you will allow me to correct an egregious lapse in my review essay on Stuart parliamentary history (*AHR*, 77 [1972]: 705-14). The sentence in question read: "Bernard Bailyn has recently stimulated a major re-examination of the American Revolution by suggesting that Locke really did matter to the colonists"—the very reverse of Bailyn's thesis. What I should have written is that *political ideas* really did matter, not Locke, and I must apologize to Professor Bailyn for misrepresenting his views.

THEODORE K. RABB
Princeton University

Other Books Received

Books listed were received by the *AHR* between May 1 and July 15, 1972. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

- ALEXANDER, EDWARD PORTER (ed., with an introd.). *The Journal of John Fontaine: An Irish Huguenot Son in Spain and Virginia, 1710-1719*. Williamsburg: Colonial Williamsburg Foundation; distrib. by the University Press of Virginia. 1972. Pp. xii, 190. \$4.95.
- ALVES, RUBEM A. *Tomorrow's Child: Imagination, Creativity, and the Rebirth of Culture*. New York: Harper and Row. 1972. Pp. 210. \$6.95.
- ANDERSON, M. S. *The Ascendancy of Europe: Aspects of European History, 1815-1914*. [London:] Longman. 1972. Pp. xi, 332. £1.75.
- BARLETTA, EDVIGE ALEANDRI (ed.). *La depositeria del concilio di Trento*. Vol. 1, *Il registro di Antonio Manelli, 1545-1549*. Ministero dell'Interno, Pubblicazioni degli archivi di stato. Fonti e sussidi, No. 1. Archivio di stato di Roma. Rome: the Ministero. 1970. Pp. xi, 435. L. 5,500.
- BARRINGER, RICHARD F., with the collaboration of ROBERT K. RAMERS. *War: Patterns of Conflict/Technical Manual*. Cambridge, Mass.: MIT Press. 1972. Pp. vi, 127. \$5.95.
- BORI, PIER CESARE. *Koinōnia: L'idea della comunione nell'ecclesiologia recente e nel Nuovo Testamento*. Testi e ricerche di Scienze religiose pubblicati a cura dell'Istituto per le Scienze religiose di Bologna, No. 7. Brescia: Paideia Editrice. 1972. Pp. 134. L. 2,000.
- BOWETT, D. W. *The Search for Peace*. The World Studies Ser. Boston: Routledge and Kegan Paul. 1972. Pp. xiii, 236. \$8.25.
- BUTTERFIELD, SIR HERBERT. *The Discontinuities between the Generations in History: Their Effect on the Transmission of Political Experience*. The Rede Lecture, 1971. New York: Cambridge University Press. 1972. Pp. 34. \$1.65.
- CÉSAIRE, AIMÉ. *Discourse on Colonialism*. Tr. by JOAN PINKHAM. New York: Monthly Review Press. 1972. Pp. 79. Cloth \$4.95, paper \$1.45.
- DARCY, SAM. *Late Afternoon for the Nation-State: A Study of the Origins, Growth, Present Position, and Possible Future of the Nation-State as a Form of Social Organization*. New York: Cromwell Books. 1972. Pp. v, 408. \$8.95.
- DEXTER, BYRON (ed.), assisted by ELIZABETH H. BRYANT and JANICE L. MURRAY. *The Foreign Affairs 50-Year Bibliography: New Evaluations of Significant Books on International Relations, 1920-1970*. New York: R. R. Bowker for the Council on Foreign Relations. 1972. Pp. xxviii, 936. \$34.50 postpaid.
- FELDMAN, BURTON, and RICHARDSON, ROBERT D. *The Rise of Modern Mythology, 1680-1860*. Bloomington: Indiana University Press. 1972. Pp. xxvii, 564. \$19.95.
- FENNELLY, JOHN F. *Twilight of the Evening Lands: Oswald Spengler—A Half Century Later*. New York: Brookdale Press. 1972. Pp. vii, 181. \$5.95.
- GALBREATH, ROBERT (ed.). *The Occult: Studies and Evaluations*. [Bowling Green, Ohio: Bowling Green University Popular Press. 1972. Pp. 126. \$1.50.
- GARRATY, JOHN A., and GAY, PETER (eds.). *A History of the World*. Vol. 1, *The World to 1500*; Vol. 2, *Toward Modernity*; Vol. 3, *The Modern World*. New York: Harper and Row. 1972. Pp. xvi, 505; xvi, 308; xvi, 412. \$5.50 each.
- GAY, PETER, and CAVANAUGH, GERALD J. (eds.). *Historians at Work*. In 2 vols. New York: Harper and Row. 1972. Pp. xx, 431; x, 406. \$15.00 each.
- GUTERMAN, SIMEON L. *From Personal to Territorial Law: Aspects of the History and Structure of the Western Legal-Constitutional Tradition*. Metuchen, N.J.: Scarecrow Press. 1972. Pp. v, 7-326. \$10.00.
- KAVOLIS, VYTAUTAS. *History on Art's Side: Social Dynamics in Artistic Efflorescences*. Ithaca: Cornell University Press. 1972. Pp. vi, 222. \$8.50.
- KEOHANE, ROBERT O., and NYE, JOSEPH S., JR. (eds.). *Transnational Relations and World Politics*. Written under the auspices of the Center for International Affairs, Harvard University. Cambridge, Mass.: Harvard University Press. 1972. Pp. xxix, 428. \$15.00.
- KIRBY, J. L. (ed.). *Annual Bulletin of Historical Literature*. No. 55, *Dealing for the Most Part with the Publications of 1969*. [London:] Historical Association. 1972. Pp. 188. £1.

- KORS, ALAN C., and PETERS, EDWARD (eds. with an introd.). *Witchcraft in Europe, 1100-1700*. Philadelphia: University of Pennsylvania Press. 1972. Pp. viii, 382. \$17.50.
- KRANZBERG, MELVIN, and DAVENPORT, WILLIAM H. (eds.). *Technology and Culture: An Anthology*. New York: Schocken Books. 1972. Pp. vii, 9-364. \$10.00.
- KRIVATSY, PETER (comp.). *A Catalogue of Incunabula and Sixteenth Century Printed Books in the National Library of Medicine*. 1st Supplement. DHEW Publication No. (NIH) 71-296. Bethesda, Md.: U.S. Department of Health, Education, and Welfare, Public Health Service, National Institute of Health, National Library of Medicine. 1971. Pp. v, 51. \$2.75.
- KURIHARA, KENNETH K. *Essays in Macrodynamic Economics*. Under the patronage of the Economic Growth Institute, State University of New York at Binghamton. Albany: State University of New York Press. [1972.] Pp. 160. \$7.50.
- LAEBER, WALTER. *America, Russia, and the Cold War, 1945-1971*. America in Crisis. 2d ed.; New York: John Wiley and Sons. 1972. Pp. 339. Cloth \$7.95, paper \$3.50. See rev. of 1st ed. (1967), *AHR*, 74 (1968-69): 113.
- LANGER, WILLIAM L. (comp. and ed.). *An Encyclopedia of World History, Ancient, Medieval, and Modern, Chronologically Arranged*. 5th rev. ed.; Boston: Houghton Mifflin. 1972. Pp. xxxix, 1569. \$17.50. See rev. of 1st ed. (1940), *AHR*, 46 (1940-41): 847.
- LEISS, WILLIAM. *The Domination of Nature*. New York: George Braziller. 1972. Pp. xii, 242. \$6.95.
- LOUGEE, ROBERT W. *Midcentury Revolution, 1848: Society and Revolution in France and Germany*. Civilization and Society: Studies in Social, Economic, and Cultural History. Lexington, Mass.: D.C. Heath. 1972. Pp. vii, 199.
- LYONNET, STANISLAO. *Il Nuovo Testamento alla luce dell'Antico: Lezioni*. Associazione Biblica Italiana, Studi biblici pastorali, No. 3. VII Settimana Biblica del Clero, Napoli, luglio 1968. Brescia: Paideia. 1972. Pp. 149. L. 1,500.
- MARX, KARL, and ENGELS, FREDERICK. *On Colonialism: Articles from the New York Tribune and Other Writings*. New York: International Publishers. 1972. Pp. 382. Cloth \$7.50, paper \$2.65.
- MILLETT, ALLAN R., and COOLING, B. FRANKLIN, III (comps.). *Doctoral Dissertations in Military Affairs: A Bibliography*. Bibliography Ser., No. 10. Manhattan: Kansas State University Library. 1972. Pp. v, 153. \$5.00.
- MONTAGNINI, FELICE. Rom. 5, 12-14, *alla luce del dialogo rabbinico*. Associazione Biblica Italiana, Supplementi alla Rivista biblica, No. 4. Brescia: Paideia. 1971. Pp. 83. L. 1,000.
- MUENSTERBERGER, WARNER, and ESMAN, AARON H. (eds.). *The Psychoanalytic Study of Society*. Vol. 5. New York: International Universities Press. 1972. Pp. 258. \$12.00.
- OLNEY, JAMES. *Metaphors of Self: The Meaning of Autobiography*. [Princeton:] Princeton University Press. 1972. Pp. xi, 342. \$12.50.
- PITT, DAVID C. *Using Historical Sources in Anthropology and Sociology*. Studies in Anthropological Method. New York: Holt, Rinehart and Winston. 1972. Pp. viii, 88.
- RISTOW, WALTER W. (comp.). *A la Carte: Selected Papers on Maps and Atlases*. Washington: Library of Congress. 1972. Pp. x, 232. \$4.00.
- SAHLINS, MARSHALL. *Stone Age Economics*. Chicago: Aldine-Atherton. 1972. Pp. xiv, 348. \$8.95.
- SCHULZ, GERHARD. *Revolutions and Peace Treaties, 1917-1920*. Tr. by MARIAN JACKSON. London: Methuen; distrib. by Barnes and Noble, New York. 1972. Pp. vi, 258. \$12.50. See rev. of German ed. (1967), *AHR*, 73 (1967-68): 443.
- SHORT, JAMES F., JR., and WOLFGANG, MARVIN E. (eds.). *Collective Violence*. Law in Action. Chicago: Aldine-Atherton. 1972. Pp. viii, 387. Cloth \$12.50, paper \$4.95.
- SPITZ, LEWIS W. (ed.). *The Northern Renaissance*. Sources of Civilization in the West. Englewood Cliffs, N.J.: Prentice-Hall. 1972. Pp. x, 179. Cloth \$5.95, paper \$2.95.
- STEARNS, PETER N. *The European Experience since 1815*. New York: Harcourt Brace Jovanovich. 1972. Pp. xx, 476.
- STIPP, JOHN L., et al. *The Rise and Development of Western Civilization*. Vol. 2. 2d ed.; New York: John Wiley and Sons. 1972. Pp. xix, 853. Cloth \$11.95, paper \$8.95.
- WOLFE, MARTIN (ed.). *The Economic Causes of Imperialism*. Major Issues in History. New York: John Wiley and Sons. 1972. Pp. x, 184. Cloth \$7.50, paper \$3.50.

ANCIENT

COMBÈS, ROBERT. *La République à Rome (509-29 av. J.-C.)*. SUP: "L'historien," 9. Paris: Presses Universitaires de France. 1972. Pp. 200. 14 fr.

Mélanges de préhistoire, d'archéocivilisation et d'ethnologie, offerts à André Varagnac. Preface by GABRIEL MARCEL. École Pratique des Hautes Études—VI^e Section, Centre de Recherches Historiques. Bibliothèque générale. Paris: SEVPEN. 1971. Pp. xii, 735.

PALANQUE, JEAN-RÉMY. *Le Bas-Empire*. "Que sais-je," No. 1455. Paris: Presses Universitaires de France. 1971. Pp. 127.

SALONEN, ARMAS. *Die Ziegeleien im alten Mesopotamien*. Suomalainen Tiedekatemia Toimituksia, Ser. B., No. 171. Helsinki: Suomalainen Tiedekatemia. 1972. Pp. 206, 52 plates. 52 M.

Sylloge Nummorum Graecorum: The Collection of the American Numismatic Society. Pt. 2, *Lucania*. New York: American Numismatic Society. 1972. 38 plates. \$25.00.

MEDIEVAL

BASIN, THOMAS. *Histoire de Louis XI*. Vol. 3 (1477-1483). Ed. and tr. by CHARLES SAMARAN and MO-

NIQUE-CÉCILE GARAND. *Les classiques de l'histoire de France au Moyen Âge*, No. 30. Paris: Société d'édition "Les Belles Lettres." 1972. Pp. xii, 462.

HOLT, JAMES C. (ed.). *Magna Carta and the Idea of Liberty*. Major Issues in History. New York: John Wiley and Sons. 1972. Pp. viii, 192. Cloth \$7.50, paper \$3.50.

MYERS, A. R. *London in the Age of Chaucer*. The Centers of Civilization Ser., Vol. 31. Norman: University of Oklahoma Press. 1972. Pp. xi, 236. \$3.50.

ROBERTSON, WILLIAM. *The Progress of Society in Europe: A Historical Outline from the Subversion of the Roman Empire to the Beginning of the Sixteenth Century*. Ed. and with an introd. by FELIX GILBERT. Classic European Historians. Chicago: University of Chicago Press. 1972. Pp. xxvii, 185. \$9.00.

SANUDO TORSELLO, MARINO. *Liber Secretorum Fidelium Crucis super Terrae Sanctae Recuperatione et Conservazione, Quo et Terrae Sanctae Historia ab Origine & Eiusdem Vicinarumque Provinciarum Geographica Description Continetur*. Reprint of 1611 ed. Toronto: Toronto University Press. 1972. Pp. xix, 312. \$45.00.

TIPTON, C. LEON (ed.). *Nationalism in the Middle Ages*. European Problem Studies. New York: Holt, Rinehart and Winston. 1972. Pp. 116.

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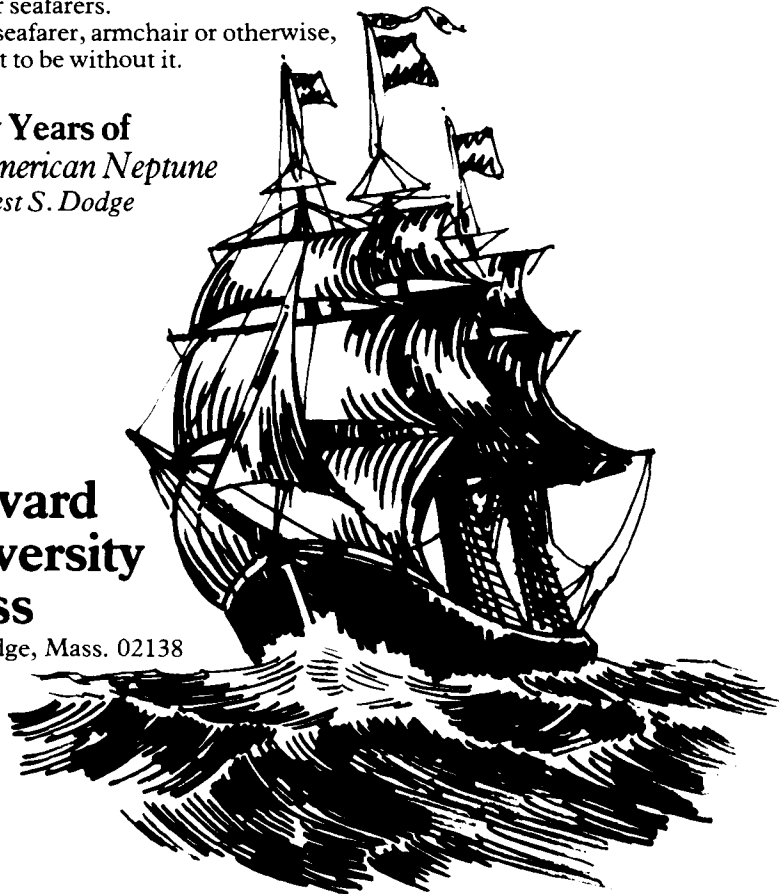
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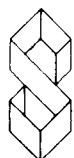
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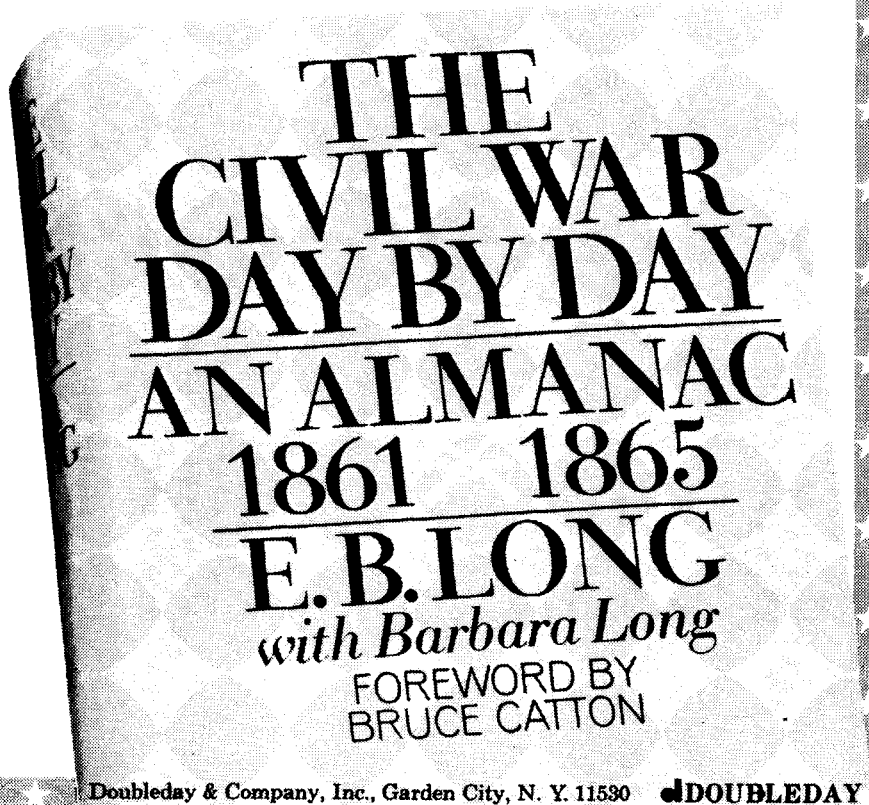
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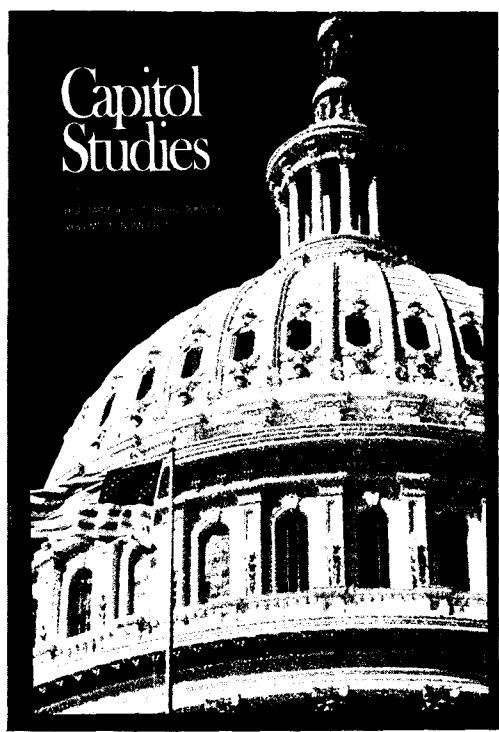
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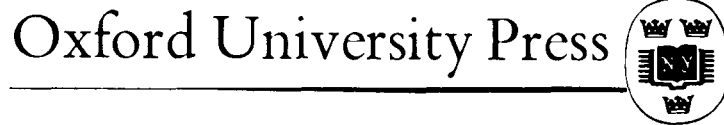
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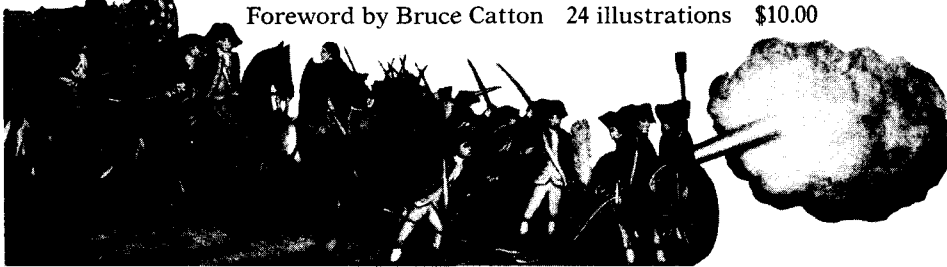
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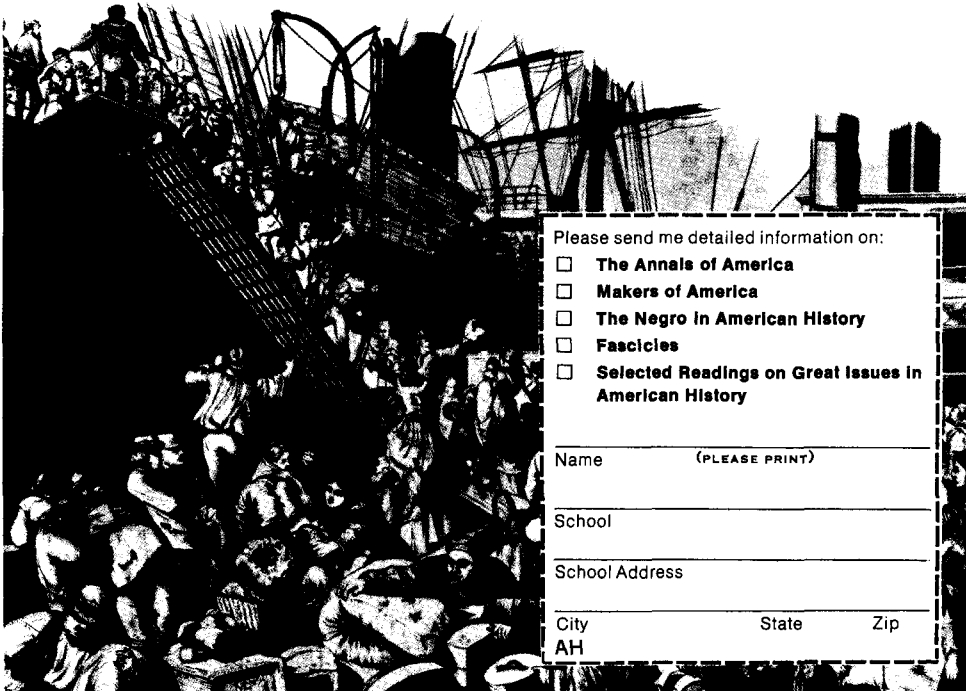
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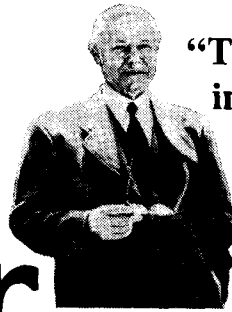
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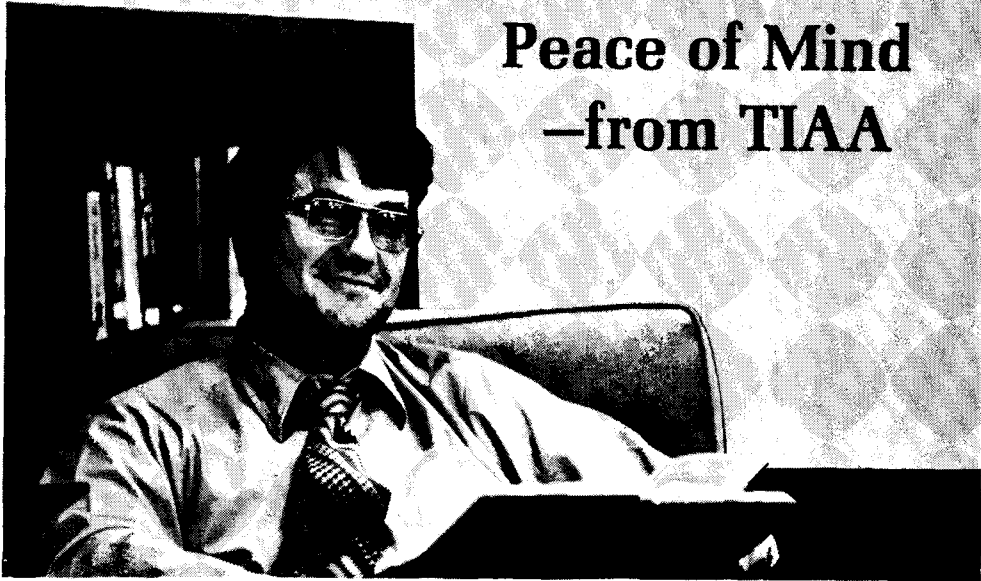
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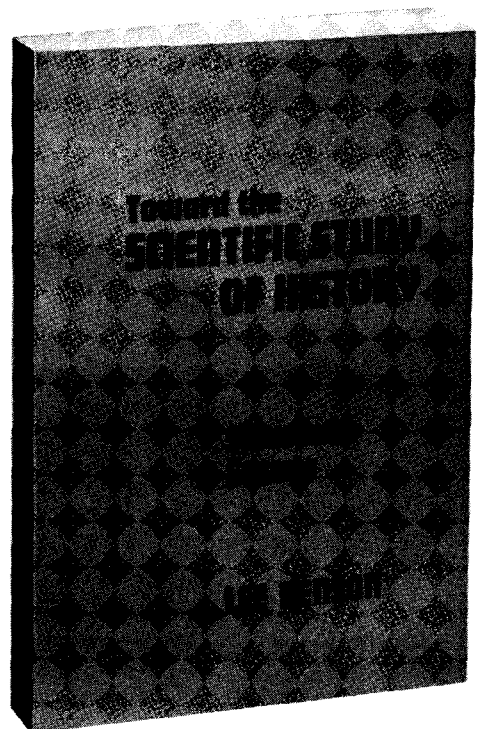
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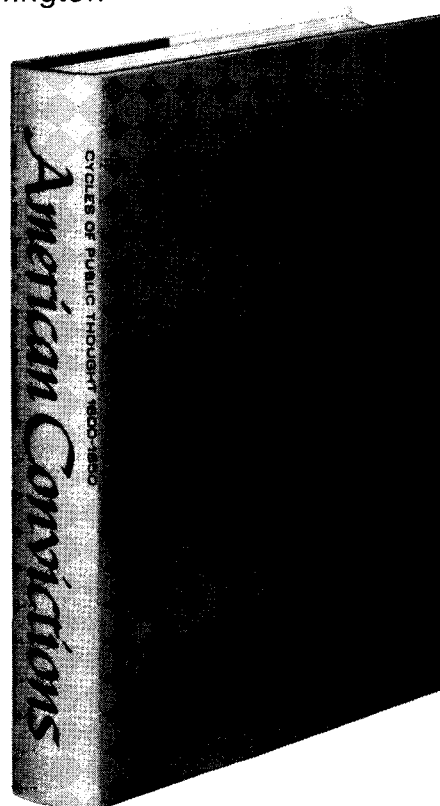
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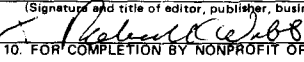
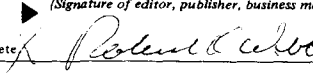
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